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OF
MEDICAL SCIENCE.

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OF

MEDICAL SCIENCE;

CONSISTING OF

ORIGINAL COMMUNICATIONS,

REVIEWS, RETROSPECTS, AND REPORTS,

INCLUDING THE

LATEST DISCOVERIES IN MEDICINE, SURGERY, AND THE COLLATERAL SCIENCES.

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AUGUST 1, 1852.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On the Application of Gutta Percha in the Treatment of Diseases of the Skin.* By ROBERT J. GRAVES, M.D., F.R.S.

WHEN a wound or injury inflicted on any portion of the surface has exposed the structures naturally covered by the skin, immediate irritation is the consequence, and this never entirely ceases until nature has been able to provide some substitute for the abraded or otherwise destroyed integument. The manner in which the denuded surface is covered with a temporary protection, sufficient to guard it against the contact of the air and other external injurious agents, varies according to the circumstances of the case: in many instances blood coagulated on the wound adheres firmly and remains in contact with every point, until the reparative efforts have healed the parts and covered them again with a new skin. When such a process takes place favourably, this most simple of all dressings

effects a cure without any external discharge or inflammation, and enables the parts to be re-formed, or, as the late Professor Macartney termed it, remodelled *by the first intention.*

Of late years surgeons have derived much advantage from the use of different imitations of this natural process, and have employed at first solutions of collodion, and finally gutta percha dissolved in chloroform, for this purpose. Reflecting upon the great superiority of this method over the others formerly used, it struck me forcibly that it was capable of a much more general extension, and that the chloroform solution of gutta percha might be usefully employed in the topical treatment of cutaneous affections. The more I thought on this subject, the more sanguine I became that this new agent would form a useful addition to the list of remedies employed in combating a class of diseases confessedly so obstinate and so difficult of cure. The result of repeated trials, I am happy to announce, has not disappointed my anticipations, and consequently I now feel myself called on to lay before the profession a statement of those cases in which this application has either effected a cure, or mainly contributed to the successful treatment of the complaint.

When the saturated solution of gutta percha in chloroform is spread by means of a camel's-hair pencil over a portion of the skin, the solvent fluid rapidly evaporates, leaving a delicate and extremely thin pellicle of gutta percha firmly adhering to the part. The peculiar toughness of gutta percha prevents this pellicle from being brittle, and therefore it is much less liable than collodion to crack and fall off in small scales. On the forehead or face, where it is not affected by friction of the clothes, it remains firmly attached for five or six days, or even longer, but on other portions of the surface it is often rubbed off much sooner; over dry eruptions of the skin it lasts longer than over those which are moist, and over smooth and firm spots, of course, longer than over those covered with rough morbid scales, or loosely adhering crusts. Before the application of

this solution, therefore, the practitioner will do well to render the portions of the cutaneous disease to which he intends to apply it as free as possible from crusts or scales, by means of poultices, alkaline lotions, &c. When this precaution is taken, he will find that the artificial cuticle which he has applied with his brush will in certain cases act most sensibly on the subjacent disease, diminishing inflammation and its consequences, and powerfully contributing to the restoration of the healthy structure of the skin.

Whether it acts by the equable pressure which the film of gutta percha, suddenly coagulated from the fluid state, must exert on the surface to which it becomes so firmly adherent, or whether its efficacy be rather owing to other causes, it is beside my object in writing this notice to discuss. I may, however, remark, that the curative properties of such an artificial covering are not confined to the skin, for I am informed by Mr. Moore, that in the "Gazette Médicale de Paris" (April 10, 1852) is an account of the successful application, by M. Dechange, of collodion in orchitis, and the *modus operandi* is said by him to consist in its compressing the tissues, and protecting the parts from the action of the air which he thinks is a powerful element of phlegmasia. Without giving an unqualified assent to this explanation, I can have no hesitation in announcing my firm conviction, that in diseases of the skin this solution furnishes us with a new and active remedy, exerting effects quite different from those produced by any topical application hitherto in use.

The transparency of this artificial membrane enables us to watch the progress of the subjacent diseased skin, and its colourless nature prevents it from disfiguring the face when the eruption occupies that part. Its perfect cleanliness, too, is no small advantage, and affords a very agreeable contrast when compared with the usual ointments, &c.

My observations confirm what reasoning on this subject would lead us to expect, that this application is more suited

for dry, scaly, tubercular, and chronic diseases of the skin, than for acute affections attended with much oozing of fluid and comparatively active inflammation.

Still, its good effects are by no means limited to chronic diseases of the skin or to those of a scaly, dry nature, for, as will hereafter appear, I have seen it decidedly useful in the spreading form of impetigo. My experience of this remedy makes me very anxious to witness its application in the first stages of erysipelas, as analogy leads me to hope for good results in such cases.

Of course the patient must aid the efforts of the physician, and must as far as possible abstain from everything which tends to rub off or injure the artificial cuticle, for its virtue ceases when its continuity is broken and the external air finds admission to any part of the diseased surface.

Early in the month of November, 1851, I was called to visit Mrs. C., from ————. She was about fifty years old, full and plethoric; the mother of a large family, and, until the disease of which she then complained commenced, generally healthy. About two years before, she observed small spots of impetigo on her limbs and body, which succeeded each other, some healing while fresh ones appeared. During summer she was nearly free from them, but last autumn they returned with greater virulence than ever, and have since increased both in size and numbers, some being larger than the hand, and attended with constant oozing of fluid, which imperfectly coagulates, forming loose and thin crusts. The itching at night is intolerable, and nearly deprives her altogether of sleep. I employed the usual general and topical treatment for a fortnight without alleviation of her sufferings, when luckily I thought of trying the saturated solution of gutta percha in chloroform, and had it carefully applied by Mr. Nicholls of Dawson-street, at first by way of trial, to one of the smaller spots, and on the following days to each of the larger patches of eruption in succession. The relief obtained was such that

it appeared almost incredible both to the patient and her family. Her cure was accomplished in less than three weeks; for, dreading the sudden stopping of so great a discharge and so much cutaneous irritation, I proceeded cautiously, and towards the end of the cure, when she returned home, I directed an issue to be inserted in her arm, as a measure of precaution. She has continued well up to the present time (11th April, 1852).

In this patient we were at first obliged to re-apply the gutta percha every second day, as it was rapidly detached and broken up into large flakes by the discharge from the subjacent surface. Its healing influence, however, speedily diminished the diseased secretion, and then the artificial cutiele remained longer adherent, and it was not necessary so often to use the scissors for the purpose of cutting off the loose portions of gutta percha membrane previously to applying a fresh layer. I ought to have remarked that the camel's-hair brush should be plunged, the moment it has been used, into hot water, to prevent it from being consolidated by the coagulated gutta percha.

This case caused a great sensation among my patient's friends and relatives, and many were the inquiries made relative to my method of cure. I must confess that my own astonishment at the result was not less than theirs.

Since that time I have repeatedly used this application in *acne of the face*, in which disease each of the pimples should be covered with the solution, and the patient enjoined not to rub off the pellicle by washing, &c.

In some this treatment alone causes a material and rapid diminution of this tormenting eruption, and by perseverance in this plan, there is every appearance in two of my patients that the tendency to throw out the pimples is gradually ceasing.

Finally, in several cases of psoriasis I have applied this solution with great benefit. In this disease much care must be taken to prevent the application being rubbed off by the

clothes, and no woollen stockings or rough garment of any sort should be allowed next the skin. I had the satisfaction of curing in a fortnight a chronic psoriasis of the back of the hands and arms in a lady who had been under homœopathic treatment for six months without deriving the least advantage from the infinitesimal doses prescribed by the practitioner.

Since the preceding observations were written, I have received the following note from Mr. Moore, a gentleman distinguished alike for his practical, scientific, and literary attainments. This additional information concerning the effects produced by the application of collodion strongly verifies my observations respecting the local antiphlogistic action of a gutta percha pellicle:

"M. La Tour has for some time advocated the doctrine that external inflammation may be speedily subdued by withdrawing the affected surface from the influence of the air. This he effects simply by a layer of collodion, which, he says, rapidly assuages the inflammation of gout and articular rheumatism. At the meeting of the Academy of Medicine, on the 8th of February, M. La Tour related a case of peritonitis in which the symptoms were dissipated within twenty-four hours, by the application of a layer of collodion to the whole surface of the abdomen"[a].

Postscript, *June* 7, 1852.

My anticipation respecting the utility of an artificial cuticle applied over the parts affected by commencing erysipelas, has, I find, been verified, as appears from the following paragraph taken from Dr. Neligan's able Treatise on Diseases of the Skin[b].

"Acting as an impermeable varnish, and probably producing some effect, also, by the compression it causes, collodion has been successfully employed by Spengler and Rapp, as a local application in erysipelas. The parts are thickly coated with it by means of a camel's hair pencil, and it is re-

[a] Bulletin de l'Académie Nationale, 1852. [b] Page 48.

newed as often as may be required in consequence of its cracking and peeling off when dry."

When my friend, Dr. Stokes, heard of my success in other cases, he resolved to try the gutta percha solution in small-pox, and it gives me great pleasure to say, the result of two trials is most encouraging, and leads us to hope that at length a means of preventing the formation of disfiguring scars on the face in that disease has been discovered. Dr. Stokes has kindly allowed me to publish the two following cases treated by him at the Meath Hospital, and noted by his diligent and zealous clinical clerk, Mr. Robert P. White.

Anne Kenny, aged 18, was admitted into the Meath Hospital, May 11th, 1852. She was never vaccinated; her illness began on the 6th of May, by rigors, headach, and pain in her loins, but without any vomiting. On the 8th, the eruption appeared, and on coming into hospital the eruption was well formed and confluent on her face; the fever of a typhoid character, with considerable prostration of strength; she was much annoyed with pain and itching of her face. She was ordered wine and carbonate of ammonia mixture, and her face to be painted with a solution of gutta percha in chloroform

The solution was applied with a soft brush; the entire face being well coated with it, and after an interval of a few minutes (just sufficient to allow the previous coating to dry), a second coat of the solution was applied.

This application, she said, gave her great relief, and allayed the pain and itching. On the 13th she was much better, and the solution was again applied in the same manner.

May 22nd. She has progressed most favourably; all the crusts have come off the face in large pieces, and there is hardly any trace of the disease remaining except a slight discoloration. During the whole time of her illness, from the application of the solution, her face continued moist, and there was no ulceration in any place.

Catherine Sherlock, aged 12, was admitted May 13th

(seventh day of eruption). Her illness commenced by headach, pain in her loins, and rigors, but without sickness of the stomach; she was never vaccinated. On admission she had a good deal of fever of a typhoid type, the eruption had come out well, was confluent, and had completely covered her face; she complained very much of pain and itching of her face and head, which kept her from sleeping. The solution of gutta percha was applied as in the former case, and apparently gave her much relief, for she was easier afterwards, and ceased rubbing and tearing her face in the manner in which she had been doing before. Wine was also freely used, as in Kenny's case.

May 22nd. All trace of the disease has nearly gone, the crusts coming off in large pieces, and the remaining marks of the disease being very trifling when compared with the violence of the attack.

The same effect of the solution in keeping the face moist was also observed in this case as in the preceding.

In communicating these cases, Dr. Stokes observed, as worthy of notice, and probably connected with the beneficial result produced, that the most remarkable effect of the gutta percha was to keep the face constantly moist, and to prevent the formation of hard, irritating crusts. He also mentioned to me a singular illustration of the effects of total exclusion of air from the cutaneous surface, as a preventive of the eruption in small-pox. It was that of a man who, while in the Meath Hospital for a scrofulous enlargement of the knee-joint, was attacked with this disease; the knee had been previously tightly strapped with adhesive plaster, and on the disappearance of the eruption, it was seen, on removing the strapping, that not a single pustule had been developed on the parts which were thus covered.

It is scarcely necessary to add that, however useful solution of gutta percha in chloroform, or any other topical applications, may prove, it would be, nevertheless, absurd to ex-

pect from them a radical cure of those chronic cutaneous affections which arise from constitutional causes, whether strumous, syphilitic, or cancroid; as well might we look for the cure of scarlet fever, small-pox, or measles, by means of remedies applied to the skin solely with the view of relieving the eruption. Still, in every chronic disease it is manifestly desirable to diminish, by topical means, the annoyance or disfigurement arising from the cutaneous affection. This relief cannot but be serviceable if it did nothing else than afford time for the due and gradual employment of a well-directed constitutional treatment. I saw this lately well exemplified in the case of a young lady who had been under the care of an eminent London surgeon for many months, and from whose medicines she had derived no relief. She was the subject of a most disfiguring eruption, agreeing in character with the lupus serpiginosus of late writers; it occupied sparsely the nostrils and cheeks, but on the forehead it formed an extensive group of sores. Some spots it left hypertrophied after healing, but in general its course was marked by white depressed seams. It had been the seat of constant irritation, which the patient could not avoid increasing by scratching during sleep, and consequently the surface of the face usually exhibited a number of bloody points, adding to its disagreeable appearance. The entire eruption healed after a six weeks' application of the gutta percha, and she is now (what she had not been for two years) free from any cutaneous irritation. Time alone can determine whether relief will be permanent, but there can be no doubt of the great value of the advantage already gained.

I shall conclude with observing that, in psoriasis and other chronic cutaneous complaints unattended with any constitutional derangement, it is of the greatest consequence to check the growth of each new spot. This the gutta percha does most effectually in psoriasis, and when applied daily to any recent points of irritation, it smothers, as it were, each nascent centre of future blotches.

Art. II.—*On the Injurious Effects arising from the Manufacture of Lucifer Matches, as observed in the Neighbourhood of Manchester.* By James Bower Harrison, M. R. C. S. E., formerly Surgeon to the Ardwick and Ancoats Dispensary, &c.

As the manufacture of lucifer matches is a comparatively new branch of trade, it is important that its effects upon artisans should be properly understood, and every means afforded them of protecting their health and their lives. It will, unfortunately, be found that when the trade is practised *under disadvantageous circumstances* the most formidable and appalling evils arise from it. An affection ensues which is so insidious in its nature that it is at first supposed to be a common toothache, and a most serious disease of the jaw is induced before the patient is fairly aware of his condition. This disease gradually creeps on until the sufferer becomes a miserable and loathsome object, spending the best period of his life in the wards of a public hospital, and these evils are incurred for a remuneration as small as that of six or eight shillings per week. It will be seen that most persons quit the employment before the disease has proceeded far, so that the employers are not always aware of the extent of the mischief. Many patients have died from the disease; many, unable to open their jaws, have lingered with *carious* and *necrosed* bones; others have suffered dreadful mutilations from surgical operations, considering themselves happy to escape with the loss of the greater portion of the lower jaw. That such results call for the investigation of the philanthropic surgeon there can be no doubt, and it equally becomes the duty of society to give a proper consideration to the information which is afforded.

My attention was first directed to this subject during a visit which I paid to the Manchester Infirmary, where I saw a young female suffering from the complaint. She would have been good-looking if it had not been for the foul disease of her jaw.

I was told at the hospital that fourteen patients could be enumerated who, in the space of a few years, had been admitted with the same disease, and, I believe, from the same manufactory. I do not imagine that the evils which attend the manufacture of lucifer matches are commonly known, and I am very sure that it is necessary they should be so. I think it would be well if the manufactories in which the trade is carried on were subjected to suitable inspection and stringent regulations. How great is the importance of the ventilation of these places will, I think, appear in the sequel. I trust, however, that the results which I am about to describe are those of exceptional cases, but it is still proper that such exceptions should be known. I do not wish to censure those engaged in the trade: some, I am aware, who had entered upon it without a full knowledge of its consequences, are now seeking to ameliorate the condition of their work-people. I wish rather to make it apparent that these matters should not be left to the knowledge or benevolence of individual manufacturers; for who can say how many ignorant persons may yet embark in the trade? Who can say how many persons of limited capital may commence the manufacture in small and ill-ventilated chambers. The brave soldier is not forgotten by a grateful country, and shall the sons and daughters of industry perish ignominiously by the labour of their own hands? When I thought of inquiring into the extent of the mischief which resulted from the manufacture of lucifer matches in this neighbourhood, I obtained some particulars from patients who had been formerly treated in the Manchester Infirmary. As most of the sufferers had, however, been removed to their own homes at some little distance from the town, I could not readily collect all the information I desired, particularly as the weather was extremely wet at the time, and I became unexpectedly engaged with other and important occupations. I am proud to say that the operatives with whom I obtained an interview made no reproachful allusions to the nature of the employment, and spoke

in a modest and resigned manner that would have done credit to the most educated of men. I have given the statements of the patients, without any pretensions to offering full details of their cases, and much in the way that the evidence of a witness might be recorded; the repetitions which occur have been purposely suffered to remain, and I have confined myself chiefly to such remarks as appear to elucidate the effects of the employment. It was my original intention to have spoken of all the fourteen cases, but the few I have given may, in the present instance, be sufficient.

Case I.—Anne Mac Vie, aged 20, was occupied four years before she suffered from her employment. Her teeth were previously good; her complaint began like ordinary toothache, and was considered by her as such in the commencement. She was occupied only in putting the lids on the boxes; at first she could perceive the smell of the preparation, but afterwards she became insensible to it; she stated that her clothes, as well as her hands and arms, were incandescent at night; she had enjoyed good health previously to being engaged in the occupation, and had been employed in a silk factory. She had not taken her meals in the lucifer match factory, and had always been in the habit of washing her hands, and attending to personal cleanliness. When she uncovered her face I perceived that the lower jaw was nearly deficient, and at the side of the jaw were two or three large holes. Mr. Jordan had removed the jaw, at the Manchester Infirmary, about seven years ago (January, 1852). She is now occupied in putting labels on boxes; her wages had been six shillings per week; she is of a pale and scrofulous-looking appearance; she had been accustomed to have meat daily, at least as much as her wages would admit of. She said that she had no gathering about the tooth in which the disease began, but that the complaint was at first like common toothache; she resides in the Droylsden Road, Newton Heath. She says that many of the work-people take their meals in the work-rooms. (The matches made are

of wood: the dippers dip the matches in bundles into the preparation contained in iron bowls, and they are afterwards spread out to dry.) She was seven months in the Infirmary; altogether her appearance is a sad spectacle. She had no salivation previously to the disease of the jaw, but afterwards a profuse one; she cannot now chew anything that is not quite soft. Says she works at what the people term "*piece-work*," that is, she obtains remuneration according to what she does, it being sometimes six, sometimes seven, and sometimes eight shillings per week.

Case II.—Margaret Burns, resides at Newton Heath, next to Mr. Woolstencraft's timber yard; aged 23; employed about two or three years previously to her suffering from the occupation. Her department was that of picking (sorting) the matches previously to their being put into boxes after they were dipped. She had good teeth before entering on this business; the complaint began like ordinary toothache, for which she had one tooth extracted, but the gum afterwards gathered and discharged outside; she could at first smell the peculiar fumes, but not after a time; she had good health before she entered the employment; she did not take her meals in the work-place, but at home, as she did not live far off; it is now (January, 1852) four years since she was affected; she had not been in any occupation before entering the factory; she always washed her hands before her meals, and was attentive to cleanliness. She had heard something of the injurious nature of the work before she commenced it, but conceived that the reports were exaggerated, and that she should escape. She obtained from seven to eight or ten shillings per week, according to what she did; she states that formerly the business was carried on in two rooms, but that now they were thrown together, and all worked in the same room, which did not smell so bad as each did formerly; she is not of a scrofulous appearance, and would be good-looking if it was not for her complaint. The lower part of the face is much altered by the loss of the greater

portion of the jaw, which gives a peculiar cast to the countenance, the natural oval of the face being destroyed; she had been in the habit of getting meat daily, and had previously, and now has, good health, irrespective of the state of the jaw. She was taken to the workhouse after the appearance of the complaint, where she was under the care of the resident surgeon for one year and a half; she said she underwent three operations at the workhouse; she was subsequently four months in the Manchester Infirmary, where she says Mr. Jordan performed two operations; she was not subject to cough, and sufferred in no other way than from her jaw; but she states that some persons in the occupation suffered from coughs, but she believed them to be naturally subject to such affections; her appetite was not affected by the employment; the lower jaw seemed nearly all absent, and presented a shocking and revolting appearance; even at the present time (January, 1852) it is not properly healed; she had no salivation before the jaw became diseased, but afterwards a profuse one; she is obliged to live on soft food, and cannot chew anything not previously crushed and made soft. She is now employed in making boxes, and is out of the way of the fumes; for her present occupation she obtains about six shillings a week. The disease commenced in a tooth at the right side, and gum abscesses formed about it. This girl, like the other is, as regards appearance and comfort, quite ruined for life; she said that she suffered so much that she often wished to destroy herself.

Case III.—Joseph Johnson, aged 21. His face looked swollen and distorted, and on one side was the mark of a deep, round ulcer; he had worked six years in the occupation before he began to suffer, the complaint then came on like toothache; he does not believe he had any decayed teeth prior to his entering the occupation. He was a *dipper*, and held his face over the heated preparation; he worked from 6 in the morning until half-past 7, dipping all the while; had good health previously. When the disease commenced he came to

the Infirmary at Manchester; he has now no teeth in the lower jaw, which is in a great measure destroyed; he did not take his meals in the place. He obtained fourteen shillings per week for what he called *piece-work*, that is, he was paid in proportion to what he did. He mixed the preparation as well as dipped the matches; says that the preparation is composed of phosphorus, chlorate of potash, and glue, the matches being previously dipped in sulphur; the matches are of wood. Says that he was particular in washing himself, &c.; that he did not know of the injurious effects previously to his entering the employ; says that he stood all day, except at meal times, i. e., from 6 A. M. to 7 P. M.; he lived near the establishment, and could smell the fumes even out of the building when the wind blew in a favourable direction; during all the period of his occupation he could smell the odour of the preparation; his clothes became incandescent with it; and the rooms in places looked like white smoke; he had a good appetite whilst in the occupation, and was well in all other respects, not having cough, &c. Says he knows of fourteen who have suffered from the work, all residing at Newton Heath, and that two died. He had pains in his knees from long standing, and the knees became in some measure crooked with it; he is well now, but cannot chew anything hard; he works at present in making cases for packing up, for which he obtains 8*s.* 6*d.* per week; he works from 6 A. M. to half-past 7 P. M.; he had salivation after the disease set in, but not before it; he cannot say that his taste was affected, or that he perceived the phosphorus in his mouth. Does not think the place is damp; none of the people made any reflection on the master.

CASE IV.—Anne Farrell, aged 19, at present in the Manchester Infirmary with diseased jaw. Said she got remunerated according to the work she did, which did not bring more than seven or eight shillings per week; that she was engaged in separating the matches; that she was not aware of having any decayed teeth when she went to the works, but that the com-

plaint commenced like toothache; afterwards the gum swelled by the side of the tooth, gathered, and burst; a discharge took place; the swelling then re-formed and again burst; that her face was not swelled when she left the establishment, but she left from her knowledge of what had happened to others. This was her first place of occupation; she had not previously been in any employment; she had not perceived any peculiar bad taste or odour, but this she attributed to being accustomed to the rooms; said that formerly the rooms were very close, but that they had been lately altered so that two rooms were thrown into one; she stated that her clothes were incandescent at night; said that young men were employed in the dipping department, but that no women were employed in it; several of the former, however, had suffered.

Case V.—Bridget Ryan, aged 15, also in the Infirmary, with diseased jaw; formerly worked in a cotton manufactory; has been three months in the Infirmary, and had been ill four months before. The rooms were formerly close, but have lately been altered; she did not make more than six shillings per week; she had a toothache when she left; had not any decayed teeth before she went; her occupation was to put the matches in the boxes; had been employed three years before her jaw became diseased. She says the rooms look incandescent when the gas is put out, and that her arms and dress were so whilst she was employed. I asked whether any precautions were taken to prevent mischief to the operatives. Thinking I was referring to precautions against fires, she stated in reply that the matches frequently took fire. When she got accustomed to the work, she did not perceive any particular taste or smell. Stated that several young men had suffered from the dipping department, in which young men were exclusively employed.

I was told that two persons had died from the occupation, one, J. D., aged 24, who was the nephew of the gentleman who kept the establishment. He had been in the Infirmary, but was discharged as incurable, and died at home. He had been

employed as a dipper. Another, William Reynolds, also died at his own house, was never brought to the hospital, but was under the care of Mr. Pegge; he had been employed three or four years, and was about eleven years of age; was employed as a lapper up.

In the establishment in which these cases occurred it seems that the work-people stand during the whole day, except at meal-times, and many suffer from the state of the knees. The floor is partly of brick and partly of flag. Efforts have been made, and are still being made, to prevent the fumes from being perceptible, and the place is, in this respect, now much improved. The dippers are generally lads. The hours are from 6 in the morning until 7 at night, with the exception of half-an-hour for breakfast, one hour for dinner, and half-an-hour for tea: all have the same hours. There were formerly two rooms, but they are now both thrown into one.

The period necessary for the production of the disease must obviously depend on various causes, particularly on the constitution of the employed, as to whether it is scrofulous or otherwise; something also will depend on the condition of the teeth, whether carious or sound; also on habits of cleanliness; and the department in which the person is engaged. It will be seen, however, to range generally from two to six or eight years, but in some cases mischief did not arise before eleven or more, so that immunity for a considerable period should not render us certain that no danger is to be apprehended. I collect the following on this point from some of the medical periodicals. Dr. Strohl[a] mentions several cases which occurred after various periods; the first case, which was fatal, happened in a female, it is stated after *many years'* employment. The second also in a female, aged 24, who was a dipper; the disease came on after five years of occupation, and commenced *three months after she had left,* when eight teeth became ra-

[a] American Journal of Medicine, 1846.

pidly carious; the upper jaw became diseased as well as the lower, and the matter (pus) had the odour of phosphorus. The third case, a female of 22, occurred after two years' employment; her health had continued good until the last two months of her employment, the gums then began to soften and swell, violent pains came on in the teeth of the upper jaw, the teeth became *carious* and fell out, a fetid suppuration arose, and the bone became extensively diseased.

The next case was likewise in a young woman of 22. Her department was to make up parcels, but *this was done in the same room where the matches were dipped;* at first she experienced a defluxion from the nose; after three years the face began to swell, and she left the business; the upper jaw became carious after the formation of an abscess.

M. Sedillot, of Strasburg, relates three cases which came on after about four years, all females, one died.

In the manufactories at Vienna, cases did not occur until the employment had been pursued for eleven years.

In Mr. Taylor's cases[a], the first patient, a man of 49, had been engaged eight years; the complaint commenced with pain in the upper jaw, and purulent discharge. The second, a man of 45, had been fifteen or sixteen years in the employment; he was a kind of superintending assistant, and had the credit of being the first who introduced the trade into England; he was of intemperate habits.

Dr. Ebel has furnished an account of the result of five years' experience in the factory at Waldmichelberg, as well as a digest of what had been previously written on the subject. It would seem that the factory where his observations were made was under peculiarly good management, and, in all probability, the ventilation was very perfect. There were about 200 individuals employed there, both male and female, chiefly between fourteen and eighteen years of age. The hours of

[a] Lancet, vol. ii. 1849.

occupation were from 5 to 7, two being allowed for meals, the meals not being taken in the work-place. Attention is also paid to the health of those admitted into the establishment previously to their being engaged. There is less wonder, therefore, that the place is stated to be free from disease. It appears that caries of the teeth is common in the neighbourhood, but not more common in the establishment than elsewhere. Out of 200 persons 153 had some carious teeth. Dr. Ebel states that the health of the persons in the factory was on the whole better than that of those who were in a similar rank of life out of the employment, and that, after a few years, an improvement was remarked in their condition. The work, he says, is not laborious, and the rooms in which it is conducted are spacious. It is incidentally remarked that some of the diseases of the locality, especially the itch, became less prevalent since the establishment of the factory, a result which may very likely arise from the sulphurous fumes. On the whole, this report is very favourable; but perhaps it is a little premature, for the proprietor does not seem to have had more than eleven years' experience. It shows, however, how much may be done by a good system of ventilation and cleanliness, with spacious rooms[a].

In concluding these observations it may be interesting to notice briefly some of the suggestions which have been made for the prevention of these injurious effects.

Mr. Henry Taylor, of Nottingham, suggests that a mask be worn over the face of the dipper, with a tube to pass out of the building into the open air, provided with valves for exhaling and inhaling, like those attached to the chloroform inhalers[b].

In a report of an operation performed by Mr. Stanley of St. Bartholomew's Hospital[c], the writer says: "The painful feelings excited by the sight of this new victim of our vaunted improvements in various branches of manufacture were greatly

[a] Casper's Wochenschrift, 1851, Nos. 10 and 11.

[b] Lancet, Nov. 10, 1849, p. 498. [c] Lancet, Nov. 17, 1849, p. 429.

mitigated by the announcement Mr. Stanley subsequently made, regarding the *prophylaxis* of these dreadful affections. Workmen in lucifer match manufactories have now a chance of escaping the baneful effects of the evolution of phosphorus acid, by placing saucers filled with oil of turpentine about their work-rooms; as the oil of turpentine is a solvent of phosphorus, it is expected that it will absorb the vapours which cause so much mischief. This precaution is taken at a large lucifer match manufactory in the neighbourhood of the London Hospital, and the very best results are expected from it."

Dr. Baur proposes the use of alkaline fluids in addition to other means[a].

" Les moyens préservatifs consisteraient:—

" 1°. A empêcher l'action des vapeurs phosphorées sur les dents par un masque, ou mieux encore par un mouchoir, au quel serait fixée un éponge imbibée d'eau ou d'une liqueur alcaline (soude ou potasse); on empêcherait ainsi ces vapeurs d'arriver au nez et à la bouche des ouvriers.

" 2°. Dans les ateliers, il serait facile d'avoir des vases plats, remplis d'une lessive alcaline qui attireraient les vapeurs phosphorées.

" 3°. Si les individus qui travaillent dans cette industrie présentent des chicots ou des dents creuses, il faudrait faire usage d'un mastic dentaire qui comblât ses cavités. En même temps on recommendera à tous les ouvriers de soigner leurs dents qu'on leur fera nettoyer souvent, ainsi que le visage, avec un liniment calcaire. (Aquæ calcis, 30 gr.; ol pap. albi, 15 gr.)."

It is possible that the use of alkaline remedies may be of some service, but I think more is to be trusted to ventilation, to the proper construction of the rooms, to a perfect separation of departments, to an abridgment of the hours of labour, and the choice of workmen and women who are not of scrofulous

[a] Nouvelle Encyclographie, Aout, 1850, p. 219.

constitutions, and who have not decayed teeth. The habits of the work-people ought also to be carefully attended to, and no one should be allowed to take meals in the work-place. It is not unlikely also that the more injurious parts of the occupation might be rendered innocuous by mechanical contrivances which should supersede the dipping of the matches by men stooping over the fumes of melted phosphorus. But these means require not merely being known, but being enforced, and it is to contribute to such a result that I have been induced to offer the foregoing remarks.

But let me return from these details to the consideration that the poor persons whose cases I have named are still suffering from their affliction; that there are yet patients in the wards of the hospitals, who are lingering day and night with this slow and disgusting disease. Young women who were once good-looking are shut out from the prospects which marriage might have afforded, and young men who were once buoyant in health have broken their strength and spirits in struggling with a horrible disorder. Who shall take up their cause if every right-minded man does not feel it to be his own? Who shall commiserate such misfortune if every kind-hearted woman does not remember that there is no social position so elevated as to be freed from the obligations which charity imposes; and none so dignified that it is not rendered more graceful by the spirit of universal love?

APPENDIX BY THE EDITOR.

[The disease thus graphically described by Mr. Harrison is of so serious a character, its prevention of so great importance with reference to a manufacture which affords useful employment to young persons at an early age, and its history so little known to the profession, that we were anxious to ascertain if it had been witnessed in this city. We therefore personally visited the only two lucifer match manufactories which at present exist in Dublin, those of Mr. Kelly in Clarendon-street,

and of Mr. Reaney, in Upper Abbey-street; neither of them have been in operation for more than six months, but in the former, one of the lads employed in *dipping* was previously engaged for a year and a half at the same work in a factory which was in Kennedy's-lane, but is now closed; and in the latter there are two lads, aged about 19, both of whom were for several years in one of the manufactories near Manchester referred to by Mr. Harrison.

In Mr. Kelly's establishment three different manufactures are carried on under the same roof, viz., the making of ink, of blacking, and of lucifer matches; the building is large and well ventilated, and the odour of phosphorus, even in the room in which the matches are dipped and made up in boxes, is but trifling, and not in the least complained of by the boys, about twenty in number, who work there. The process of dipping differs somewhat from that described by Mr. Harrison: the compound, which consists of glue, chlorate of potash, phosphorus, and a small quantity of camphor, the latter ingredient being used to neutralize the smell of the phosphorus,—which it does very effectually,—is first melted over a slow fire, and then poured on a heated stone slab, so as to form on it a layer of about the eighth of an inch in depth. The matches, previously tipped with sulphur, and fixed loosely in a wooden frame about two feet long and a foot wide, are dipped rapidly on the flag until their points are coated with the melted composition; both ends are thus dipped, the frame being quickly turned round in the hands of the dipper in order to effect this; the matches, which are thus, so to say, double, are afterwards cut across in the middle, being placed in bundles under a large cutting knife; to prevent ignition from the friction they are necessarily submitted to while being cut, their tops, coated with the compound, are moistened before the matches are placed under the knife, nevertheless they constantly take fire, and much loss is thus occasioned. The lad in this factory who had been previously engaged in Kennedy's-

lane told us that about six months after he first went to the business he was attacked with toothache, but that he got the tooth extracted, and never had any pain in his jaws or teeth since; he showed us the tooth, which he carried about with him as memento;—it was the second molar in the lower jaw, and was affected with seemingly ordinary caries; we also examined his mouth carefully; his teeth were all sound, but they were extremely and unnaturally white, of a somewhat diaphanous aspect, and quite free from tartar or other concretion; and we may remark here that the teeth of one of the lads in Mr. Reaney's factory, of whom we shall speak particularly hereafter, and who had suffered from symptoms of the malady, presented a precisely similar aspect. A man who was also engaged in dipping in Mr. Kelly's told us that he had been at the work for about three months, that at first the smell made him sick, and did so still if his stomach was empty, and that he had occasionally headach and pains in his jaws and cheek-bones, but not like toothache; his teeth were all sound: this man had evidently heard something of the disease and was apprehensive of being attacked with it. Mr. Kelly informed us that the dipping was carried on in his establishment only twice a week, and then for not longer than two hours at a time. The odour of the phosphorus was, we think, much counteracted here by that arising from the vinegar and sulphuric acid employed in making the blacking and ink under the same roof, but was nevertheless very powerful immediately over the heated flag on which the compound was poured. It is right to add, that both the master and the men laid great stress, however, on the use of the camphor as a preventive.

Mr. Reaney's factory is devoted solely to the making of lucifer matches; the rooms, two in number, are large, airy, and well ventilated; about thirty boys between the ages of six and fourteen years, two lads about nineteen years old, and three men, are constantly employed. None of them, except the two lads, have been engaged in the business for a longer period

than five months; these two lads, as before remarked, were employed in one of the Manchester manufactories for seven or eight years. One of them, Hugh Regan, a lad of much intelligence and observation, gave us the following account of himself:—He was employed for more than seven years in the —— factory, near Manchester; latterly he had been engaged in dipping, being constantly at it every day in the week, and for nearly twelve hours each day, and was paid by piece-work; he knew several of those who had lost their jaws in the business, and he recollected some who had died in consequence of the disease. (We tested the correctness of his story by asking him the names of the persons whom he had known to be affected, when, amongst others, he mentioned those given in Mr. Harrison's paper). He found his own health failing at length, two of his teeth got bad, and one of them he had extracted, the other is still in his mouth, but does not annoy him now. (This tooth, the second molar in the lower jaw, we examined; it is worn away nearly to the level of the gum, but is not black, nor does it present any hole, and has not the usual appearance of a carious tooth.) He consequently came over to Dublin, and although he has been for the last four months engaged in this factory, he has lost all the symptoms of the disease, nor does he apprehend any return of it, as he only dips now on two days in the week, and only for two hours each day; moreover he says that the place is more airy, and the smell not so bad. (No camphor is used in the compound in this factory, and the odour of the phosphorus is much more powerful than in Mr. Kelly's, although the room in which the dipping is carried on is much larger.) This lad also told us, that in England more girls are affected with the disease than boys or men, although they are not so much exposed to the fumes of the phosphorus as the latter; and this he accounted for by a habit which he said they had of constantly picking their teeth with pins, with the idea of keeping them clean; he said that several of the girls made their teeth bleed every day by thus picking

them. The other lad had been equally long at the business, but in England he was employed in pasting the boxes together, and he followed the same occupation here, with the addition of being occasionally engaged, for a couple of hours at a time, in separating the matches which adhere together at their points with his fingers: this is the employment in which those girls who are described in Mr. Harrison's paper as being attacked with the disease were chiefly engaged in, but he has never had the least symptom of it, and his teeth are perfectly sound.

In Mr. Reaney's factory the method of dipping in a large bowl is precisely similar to that followed in England. In both the factories the operatives are paid by the week, and not by piece-work; they are all in the enjoyment of robust health, and we heard no complaints of pains in the knees from fatigue, although, with the exception of about half-a-dozen boys, they all work standing. The hours of work are from 7 in the morning until 7 at night, two hours being allowed for meals, which are eaten at their own homes.

In conclusion, we have but little to add to the admirable suggestions contained in Mr. Harrison's paper as to the prophylaxis of this afflicting disease. Our observations practically bear out the importance of proper ventilation; and from them we think may also be deduced the great advantage which would be derived from a division of labour, so that none of the employed should be exposed to the phosphorus fumes for a longer period "than two hours at a time for two days in the week," the plan followed in the Dublin manufactories.]

Art. III.—*On the Epidemic Rubeola of Accra, West Coast of Africa.* By W. F. Daniell, M. D., Staff Assistant Surgeon.

Among the negro communities of Western Africa there is, perhaps, no class of maladies that exerts a more destructive influence on human life than those which assume an epidemic character; fortunately for the interests of humanity in these

regions, their predominance has been somewhat restricted, for the few that have hitherto appeared have been mostly limited to those exanthematous affections more constant to colder climes. Of the exanthematæ, however, those characterized by an adynamic type, and of a varioloid form, have for ages past acquired a fearful celebrity, owing to the great mortality and desolation which have invariably attended on their track. Of later years their ravages have been considerably checked by the more favourable appreciation and general diffusion of the practice of inoculation, under the refuge of which popular dread and timidity have in a large measure subsided. Of these pestilential visitations, that followed by the least loss of life, and, from the rarity of its prevalence, the least known in Africa, is the epidemical outbreak which I am about to describe.

It is a well-known fact that the uniform insalubrity of the western shores of the African continent has not only cramped the freedom of intercourse and commercial enterprise, but has tended to depress the spirit of scientific inquiry, for without opportunities for the study and record of an uncertain and less frequent series of diseases which a residence in different localities alone affords, every probability exists of their escape from the notice and detection of the casual observer; and as the deadly hostility of a climate, conjoined with manifold local disadvantages, precludes in the majority of instances any protracted sojourn in such unhealthy countries, we need hardly feel surprised at the scanty array of information we possess, relative to the rarer morbid affections which may be observed amongst the native tribes.

Nor to these alone are the sources of difficulty confined: others present themselves connected with the history, progress, and termination of diseases, that involve the recognition of certain distinctive differences between the milder forms of eruptive disorders and those erythematous efflorescences on the cutaneous surface, of more habitual occurrence, inasmuch as

slight dependence can be placed on the statements of local authorities, who, from ignorance or apathy, are too apt to confound or regard these abnormal conditions of the tegumentary textures, as proceeding from one and the same cause, but modified according to circumstances.

So far as my researches extend to the few treatises and miscellaneous papers professing to delineate the aboriginal diseases of Western Africa, but slight mention has been made concerning the existence of rubeola, either epidemically or endemically, in the various European settlements and other populated districts along the coast. Winterbottom, in his excellent work on the Native Africans of Sierra Leone, unfortunately could give no satisfactory account of this malady, because, as he justly remarks, it is difficult to make the inhabitants comprehend the drift of such queries. He, however, states, on the testimony of a trustworthy personage, that measles was brought on board a slave-ship some years since, when anchored off Cape Coast Castle, by one of the canoe men who was ill of the complaint at the time, and that the infection spread immediately among the slaves on board, but was speedily checked by separating the healthy from the sick, inasmuch as the invasion was mild in its attacks, and indicated a possibility of its having been imported by some previous vessel trading at the same port. Boyle is silent both as regards the history and propagation of rubeola in Sierra Leone and the circumjacent towns; and a similar deficiency may be noticed in my own work on the Gulf of Guinea[a]. Clarke, in his treatise on the natives of Sierra Leone, merely adverts to its existence in an annual table of the diseases under treatment for the year 1840–41[b]. Notwithstanding the experience derived from a sojourn of above twelve years in many of the countries of Western Africa, this morbillous epidemic never came under

[a] Reviewed in our ninth volume.—Ed.

[b] A Description of the Manners and Customs of the Liberated Africans, p. 80.

my observation until its pervasion was conspicuously manifested throughout the Accra towns in 1850.

Although the pathognomonic peculiarities of measles may be considered to be generally unknown to the majority of the negro populations on the Windward and Slave coasts, it would, nevertheless, appear that there are certain districts where, from its periodical occurrence and isolated limitation, their inhabitants have become familiarized with its phenomena, and readily detect their early approach. This tendency to an isolated exclusiveness might be well illustrated by a reference to the most intelligent people of Accra, who conclusively agree in asserting that its visitations were well known for a long series of years past, and that the epidemic had on this account been specially designated by the term of *Intobro*. During my investigations into the source and history of the disease, I was informed by the Rev. H. Wharton, a zealous and well-informed missionary at Jamestown, that it extensively prevailed in these localities in 1846, and antecedently to that date had burst forth at irregular intervals, varying from three to five years, usually following in the wake of variola, which had commonly preceded it. Though unable to vouch for the accuracy or validity of this latter statement, I can only remark that its correctness was subsequently confirmed by several of the native chiefs, whose evidence in this matter, being perfectly disinterested, I see no reason to impugn.

The first case that came under my care occurred in a private of the 1st West India regiment, named John Maddocks, aged 24, who was admitted into the hospital at Fort Christiansburg, on the 2nd November, 1850. He had recently returned with a detachment of troops from an expedition undertaken to overawe the refractory inhabitants of Adda, a town situated on the Volta, about seventy miles to the eastward of Accra. The men during their march experienced some fatigue and annoyance, arising more from the difficulties of the roads than from any opposition on the part of the natives: none, however,

were compelled to seek medical aid while thus absent. This patient was of a spare and delicate frame, and rather debilitated constitution; he had previously suffered from a sharp attack of variola in April, but recovered without any bad sequences. When first visited he complained of pain in the loins, limbs, and shins, accompanied by uneasy sensations of coldness; the face and upper extremities were overspread by a dense and slightly elevated rash, evidently approaching the stage of desquamation, and whose efflorescent whiteness subsequently afforded a singular contrast to the darker hue of the surrounding textures. The febrile prostration was moderate, and the catarrh, sore throat, and secretions from the nose equally so; the pulse averaged then, as throughout the course of the malady, about 100, soft and frequent; the tongue was furred, and red round the tip and edges; and the bowels, after several days' constipation, had become relaxed, with symptoms of gastro-enteritic irritation, by the succeeding vomiting of which a few round worms were ejected from the stomach at different intervals; diarrhœa gradually supervened, which eventually terminated in dysentery; the dysenteric sequelæ, however, were greatly modified; the tenesmus and tormina were not constant, there were less fetor and mucus in the evacuations, which, though of a dark colour, were always more or less tinged with blood, resembling rather the aspect of a simple exudation from the coats of the intestines than those sanguineo-purulent discharges proceeding from ulcerated surfaces. Under the exhibition of calomel, opium, and ipecacuanha, in large doses, accompanied by enemata of warm water, the patient progressed favourably onwards, and was soon restored to health. The features most worthy of attention in this, as in a number of cases which private practice had placed under my cognizance, were the obvious inefficacy of the various remedial means employed to check what must doubtless be viewed here as critical discharges (since they ran a determined course without the slightest interruption), and the vigorous opposition of

the system to submit to the influence of those medicinal agents which, in analogous forms of idiopathic disease prevailing in the same localities, had hitherto exerted a controlling effect.

The second admission into hospital took place on the 17th of November; the subject, John Crabbe, belonged to the same regiment, stout and robust in conformation, and about twenty years of age. In this patient the precursory fever manifested a more sthenic type; the skin was hot and painful; the eyes intolerant of light and suffused with moisture; the throat slightly sore, with a frequent cough and scanty expectoration; pulse rapid; great thirst and restlessness, &c., in conjunction with other symptoms of an inflammatory tendency. The eruption, so far as I could judge, appeared at the usual time, viz., on the fourth day, and presented small elevated points, which rapidly diffused themselves into crescentic or semilunar clusters over the face, neck, and upper extremities, and in a few days was succeeded by that meal-like efflorescence which imparted such a singular covering to the sombre cuticle. For a brief period a moderate diarrhœa ensued, but was easily relieved, and the man returned to duty without experiencing any debilitating effects.

The third well-defined case occurred in the person of John Boggis, aged 18; also a private of the regimental detachment of the 1st West India regiment, left in garrison at Fort Christiansburg. The characteristic rash, as in the other instances, was prominently developed, in passing through its consecutive stages, with the furfuraceous desquamation that usually attended the latter sequelæ, which was so remarkably distinct that its appearance might with propriety be considered as one of the best diagnostic evidences of the predominance of rubeola in the negro races. The diarrhœa was severe, but of short duration, and within a fortnight after admission the man returned to duty fully restored to health.

A few other cases appeared among the troops in December, but of such a mild and indeterminate character as to demand no

further treatment than the confinement to barracks of those so affected. In the majority the indications of the morbillous poison were denoted by erythematous eruptions, indistinctly traceable on the face and superior portions of the body, occasionally conjoined with trifling purgings; the latter being often the only symptom of the disease, the more definite being absent, so that on the whole these imperfect illustrations may with justice be classed with that variety of measles denominated *rubeola sine catarrho.*

This epidemic visitation broke forth early in October, at first in Christiansburg, and subsequently in the populations of the adjacent towns, in primary instances attacking the children of mulattos and other hybrid families, and then those of the purer negro. Of the former not a few suffered severely, but the mortality was comparatively trifling. The premonitory febrile and catarrhal symptoms, with suffusion of the eyes, sore throat, and secretions from the nose and eyes, were always more or less apparent, and when superadded to the regular vomiting, and other gastro-enteritic disturbances, with the peculiar rash and efflorescence, easily led to the detection of the malady. Bronchitis and other pulmonic inflammations, to a certain degree, followed the course of the ordinary affection, but they seldom attained any prominent development, nor did they become invested with that formidable severity which attended the dysenteric and other morbid states of the alimentary canal. So far as my experience extended, the abdominal sequelæ were not only the most fatal, but the most constant in occurrence, and to them is to be attributed the mortality that resulted. This, however, was somewhat insignificant, for out of many hundred cases the aggregate number of deaths only amounted to twenty, and they occurred in children of a tender age and sickly growth, and were induced, according to my authorities, by the exhaustion and collapse which dysentery or a protracted diarrhœa brought on. With the exception of the few cases in the garrison, or an occasional adult in the sur-

rounding districts, the disease was generally confined to the younger branches of a family, the elder being exempt, probably from having experienced a previous attack, otherwise neither sex nor age would have been secure. Since most of the regimental detachment were composed of Fante men, enlisted at Cape Coast, it is reasonable to infer that this epidemic was not so frequent at that place as in Accra, or they might have otherwise suffered in one of the prior invasions. In the missionary school at Jamestown, or English Accra, out of 104 boys and 36 girls, 14 were attacked with the complaint between the commencement of October and the end of January, viz., 5 boys and 9 girls. No deaths took place.

The mode of treatment adopted by the native practitioners consists in rubbing dry earth over the surface of the body, the patient being directed to recline on the bare ground. Bathing and all cool beverages are scrupulously avoided, and no other liquid permitted to be administered than copious draughts of warm water or medicated drinks. The troops of Fante origin evidently followed the same plan of cure, and when in hospital it was only after considerable reluctance that they submitted to the regimen required by the military regulations.

It may be deemed worthy of mention, that the European system of treatment was found to answer remarkably well in this morbillous epidemic, and ordinarily there was little difficulty in bringing most of the cases to a happy issue, the only drawbacks in the graver forms being those dysenteric sequences which, from the rapidity of their progress, demanded at once a vigorous and stringent remedy. The distressing vomiting and other symptoms of gastric irritation were usually relieved by the application of sinapisms to the stomach and præcordia, and internally by effervescing draughts of carbonate of soda and lime juice. The diarrhœa was moderated and checked by small doses of hydrargyrum cum creta and Dover's powder, and the dysentery to a certain extent by calomel combined with opium

and ipecacuanha in larger quantities. When the tormina and tenesmus became too oppressive, a simple enema of warm water, or sometimes conjoined with castor oil or with tincture of opium, produced the desired relief. The injections of acetate of lead, employed in other countries with advantage in endemic dysentery, did not, from the few cases in which it was exhibited, indicate sufficient tokens of its good effects. From the limited number of deaths, no opportunities were afforded by post-mortem examinations for ascertaining what morbid lesions had existed, and I am therefore unable to furnish any information that might throw light on the pathological variations of tropical measles in Western Africa.

Art. IV.—*On the Treatment of Albuminuria by the Administration of Preparations of Iron.* By Cathcart Lees, M.B., Physician to the Meath Hospital, Examiner in Medicine to the Queen's University in Ireland, Lecturer on the Practice of Medicine, &c.

Although the modern researches of chemistry, aided by the use of the microscope, have within the last few years thrown much light upon the pathology and diagnosis of diseases of the kidneys, particularly of the various forms attended with the secretion of albuminous urine, and usually classed under the common denomination, "Bright's disease," yet we must admit that corresponding improvements in treatment have not appeared to keep pace with our knowledge of their pathology. I wish, therefore, in the following communication to call the attention of the profession to the use of the preparations of iron, which I have employed in many cases of albuminuria during the last five years, with the best results, particularly in the early stages of the disease. Most authors, even those who have paid much attention to the subject of albuminuria, have made but slight, and in some instances no mention of the use of iron in any stage of the affection. Thus, in Dr. Bright's latest

communication on the subject, consisting of a valuable series of cases, reported chiefly with the view of comparing the efficacy of different modes of treatment in the wards of Guy's Hospital,—as tried by Dr. Barlow, under his superintendence, aided by Dr. Rees,—we find[a] that the use of iron is only incidentally mentioned along with the other modes of treatment, but no particular attention directed to it; although the effects of mercury, of antimonials, of elaterium, and of tonics, were fully tested, and separate series of cases allotted to each, yet we only find one case (the twenty-eighth) headed with the term chalybeates, and even there it is included under the antimonial treatment, so that in not even *one single case* recorded by them, has the treatment by iron received any special attention. In Professor Christison's clinical lectures on "Bright's disease of the kidney," delivered at the Royal Infirmary of Edinburgh, in March, 1851, and published in the Monthly Journal of Medical Science for June, 1851, in which the subject of treatment is freely entered into, he does not even allude to preparations of iron as suitable for any form or stage of the disease. And in the Number of the British and Foreign Medico-Chirurgical Review for April, 1852, in a comprehensive and carefully written review of the most recent works on Bright's disease of the kidney, and which, we may presume, represents the present state of knowledge as to the treatment of this affection, the only allusion made to the administration of iron occurs in the notice of the work of Dr. Frerichs, of Kiel, who recommends lactate of iron for the anemia, and states, "that he has found relief follow the use of the solution of the hydro-chlorate of iron, in the diarrhœa attendant on the latter stages of Bright's disease." Dr. Copland says, "In the *more advanced stages* of the disease, the preparations of iron, judiciously chosen, and combined with other medicines, are often more or less beneficial." And, in his excellent practical treatise, Dr. Rees

[a] Guy's Hospital Reports, second series, vol. i.

also advocates the use of iron, but only in chronic cases, and then "with a view of supplying red corpuscles to the blood, an effect produced by its use in cases of anemia generally." Professor Osborne of this city, in a valuable communication published in the Number of this Journal for August, 1851, states that "the indications for the use of iron in Bright's disease are manifest when we consider the deficiency of the blood globules in the chronic stages."

Of the several recent works on materia medica to which I have referred, that of Dr. Neligan alone recommends the employment of iron in this disease, but he refers to its use only when the affection is chronic. In the third edition of his Treatise on Medicines, at page 457, he says that "the preparations of iron are used with benefit in the advanced stages of Bright's disease of the kidney;" and he has himself informed me that, while he has experienced the most marked advantage from their employment then, he does not administer them when the disease is acute.

A perusal of the subjoined cases will, I think, prove that the administration of iron may be commenced with advantage at an earlier period than is usually supposed, and even at a time when the symptoms, as well as the condition of the urine, would appear to indicate some congestion of the kidney, a stage we cannot strictly consider as "*chronic*," and may also tend to support an opinion I entertain, that the beneficial effects of iron in this disease are not to be attributed solely to "its imparting red corpuscles to the blood." We know that this fluid undergoes other important alterations in albuminuria, evidenced not only by the rapid change in the specific gravity of its serum, and in the great diminution of its solid constituents, particularly the urea. But that these changes are not owing to the red corpuscles being drained off by the kidneys, and thus constituting a true hemorrhage from the system, is evident from the fact, that in many of the most anemic cases, with the greatest diminution of red globules, we often find the urine

remarkably pale and clear, without any red globules; so that it must be owing to the altered condition of the liquor sanguinis, which interferes with the development of the red corpuscles, that they are deficient, probably in virtue of the endosmotic law so ingeniously applied to its solution[a] by Dr. Rees. It is as a remedy for this altered condition of the liquor sanguinis that the use of iron is particularly indicated at a comparatively recent stage of the disease, and I think that the early administration of iron, as an antidote to the commencing deteriorated condition of the blood, which I conceive to be, at least in most cases, the very essence of the disease, is not recognised as it should be; nor does this medicine rank as it is entitled, among the remedies for the treatment of albuminuria in an early stage.

I shall now detail a few cases in which I have found the steady use of iron, from even an early period of the disease, to be followed by great relief to the most distressing symptoms, the improvement in the general health being so great, that the patients considered themselves as cured, and returned to their occupations, many of them being of a laborious character.

Case I.—Patrick Smith, aged 40, a coachman, of temperate habits, was admitted under my care into the Meath Hospital (February, 1849), for general dropsy; he had been attacked the night previously with convulsions, stated by a medical man who had seen him to be of an epileptiform character, but on admission he was gradually recovering from a state of stupor, and could answer questions. His face was remarkably pale and

[a] Gulstonian Lectures, 1845.

Dr. Rees asserts also that a drain may go on from the liquor sanguinis by its *extractives passing away by the urine*, without any albumen appearing, and that we may thus have another cause for the appearance of anemia, as the extractive matters tend greatly by their presence to keep up the specific gravity of the blood, and may thus account for the blood being impoverished in some of those cases of anemia in which we fail to detect any adequate cause for the symptom.—(Medical Gazette, July 25, 1851.)

ghastly, his body, and both upper and lower extremities, very anasarcous, skin dry, tongue red, pulse 76, regular, great difficulty of breathing, with muco-purulent expectoration, and rales over the entire of both lungs; he complained of pain across the loins, and also of frequent sense of vertigo; the urine was passed in natural quantity, feebly acid, of a dark brown appearance, sp. gr. 1·010, highly albuminous with heat and nitric acid; very deficient in urea. The sediment under the microscope consisted of blood-discs, renal epithelium, and casts of the tubes both entire and broken up. He had been taking mercury for the dropsy, and was under its influence when admitted. I learned that he had enjoyed good health until three years ago, when he was treated for disease of the liver, and cured; he remained well up to eight weeks since, when, after exposure to wet, he was attacked with diarrhœa, and on its subsiding his feet and legs began to swell; in a few days his body and face became dropsical, with very scanty and high-coloured urine; the heart was acting strongly, but there was no sign of valvular disease. I considered this a case of subacute congestive desquamation of the uriniferous tubes, with the secondary head affection so often met with in the disease; I therefore ordered him to be dry-cupped over the loins, and, as the bowels were confined, to take half-a-drachm of compound powder of jalap and an eighth of a grain of elaterium. This caused gentle watery evacuations from the bowels. I allowed him sago, with a small quantity of wine, and then, even though he appeared to have general mucous secretion all over both lungs, I gave him ten grains of the ammonio-tartrate of iron three times a day; this was followed by the happiest results; his breathing improved; he had no return of convulsions nor vertigo. I then gave him a vapour-bath twice a week, clothed him in flannel, and kept him on the constant use of the iron for three months, with an occasional purge of the compound jalap powder; the anasarca disappeared entirely, the urine became perfectly clear, the specific gravity rose to 1·015, and though a trace of albu-

men could still be detected, and an occasional sediment of epithelium with one or two casts of the tubes, yet the man considering himself cured, left the hospital, and was seen by one of our students within the last month working as a railway labourer, and apparently enjoying good health. I consider this case worth recording, as showing the good effects of the treatment by iron in so early a stage of the disease, even though the condition of the urine and the pain in the back indicated congestion of the kidneys, and when also the case presented the symptoms and physical signs of acute congestive bronchitis, with profuse muco-purulent secretion. Another important feature in the case is the fact of convulsions coming on while the patient was under the influence of mercury, and tends to confirm me in the opinion I have long entertained from what I have observed in similar cases, and which I find corroborated by Drs. Bright and Barlow of Guy's Hospital, in their latest report on the subject[a], who state that "mercury was not only an inefficient, but in all probability a dangerous, remedy in those cases in which the renal disease was the primary and principal affection."

Case II.[b]—Rickaby, aged 36, was admitted into the Meath Hospital, February, 1850, for general dropsy. He states that he was always a healthy man until about eight years since, when he caught cold from having worn damp clothes; a week subsequently his face became œdematous, and afterwards his whole body swelled; he was first treated in the Dispensary of this Institution, when he was bled, as also on coming into hospital, where he was under treatment for thirteen weeks, both by Dr. Stokes and Dr. Lees, and was eventually discharged cured. From that period he enjoyed good health, until seven weeks ago, when his appetite began to fail, followed after a few days by tenderness across the abdomen and lumbar region, and in a week the face, abdomen, and lower extremities began to

[a] Guy's Hospital Reports, second series, No. 1, p. 257.

[b] Reported by Mr. George Nicholson.

swell, and have gradually increased to the present time, when there is very great anasarca, particularly of the lower extremities, and scrotum, which is enormously distended with fluid; there are also ascites, and considerable effusion into both pleural cavities, indicated by dull sound on percussion from the base of the lungs up to the inferior angle of each scapula, with absence of respiration, but change of position restores the clear sound, and also the natural respiratory murmur; the urine is scanty, dark-coloured like porter, with a copious brownish sediment; it is acid; specific gravity 1·010; very deficient in urea; highly coagulable by heat and nitric acid; under the microscope the sediment appeared to consist of blood-discs, epithelial cells, nuclei, entire and broken-down casts of the uriniferous tubes, with an amorphous deposit of lithates; the pupils of both eyes are greatly dilated, but there have been no head symptoms; he complains chiefly of pain of his right thigh, and of coldness of his feet during the day, while they are, on the contrary, uncomfortably warm during the night. He was ordered the following mixture: a drachm of ammonio-tartrate of iron, half-an-ounce of syrup, and five ounces and a half of water, of which an ounce was to be taken three times a day; he was clothed in flannel, and directed to have an occasional dose of compound jalap powder, and a vapour-bath twice in the week. Under this treatment he improved rapidly; the effusions in the chest and abdomen, as also the anasarca, disappeared; the urine became quite clear and the specific gravity rose to 1·020; urea re-appeared in natural quantity, albumen disappeared, no tubular epithelium could be detected by the microscope, and he left hospital on March 29th, having been taking the iron daily for nearly two months, apparently quite recovered, able and willing to resume his work as a labourer.

Case III.[a]—Margaret Holmes, aged 34, mother of nine children, healthy-looking, was admitted into the Meath Hospital March 4th, 1852, with general anasarca; she was in the

[a] Reported by Mr. Ulysses Fitzmaurice.

fifth month of pregnancy. When four months pregnant of a child, now three years' old, she observed her abdomen to be swelling rather unnaturally fast, but was not in any way indisposed; her urine, however, was, for a short time prior to the swelling, scanty and high-coloured; for two or three months previously to her confinement she became affected, for the first time, with a sensation of numbness almost amounting to pain across the loins; at this period her legs and feet began to swell; all these symptoms disappeared in nine days after her confinement; after this she continued in perfect health for one year and a half, when, being in her fourth month of pregnancy, the former symptoms returned in the same succession; her urine again becoming scanty and high-coloured; the swelling of her feet was at this time so great that the integuments of one of them gave way, and a large quantity of fluid exuded from it. The swelling did not leave her at this time, as before, after her confinement.

About six weeks or two months after being confined, the anasarca continuing still, she felt some dimness of vision, and got a fit, remaining unconscious for two days; on the third day her senses were again perfect: she then went to Steevens' Hospital, and, after remaining there for five weeks, she left it quite recovered.

From this time (March, 1851) to June there was no return of her complaint, but then the swelling commenced again, beginning in her abdomen, and extending from thence to the legs and feet, gradually increasing up to the present time (March 4th, 1852), attended with a sense of numbness in the back; scanty and high-coloured urine, of specific gravity 1·010, coagulable by heat and nitric acid, with a dark brown sediment, consisting of blood discs, renal epithelium, casts of the uriniferous tubes, and some mucous globules. There was general anasarca, with ascites; she suffered greatly from dyspnœa, caused, I presume, by the distention of the abdomen, for there was no disease of the heart or lungs to account for it. I had her

dry-cupped in the loins; and she was ordered to take an eighth of a grain of elaterium, and half-a-drachm of compound jalap powder, this acted gently on her bowels with great relief; I then put her on the electuary of the tartrate of iron, followed by the ammonio-tartrate of iron; under this treatment she improved rapidly, all anasarca disappeared, the albumen in the urine greatly diminished, the specific gravity rose to 1·015, with a fair proportion of urea, and she left the hospital in about six weeks, apparently perfectly recovered.

This case is interesting as illustrative of the influence of the puerperal state, not only as a predisposing, but also as an occasional cause, in exciting an acute attack of dropsy. The woman lived always comfortably and temperately, had never scarlatina, nor any symptom of inflammatory disease; but during each pregnancy the dropsy gradually returned, with scanty high-coloured urine.

I am indebted to my friend Dr. Osbrey for the details of the following interesting case.

Case IV.—A.B., a young gentleman, aged 12, had been complaining for six or eight weeks, of languor, loss of appetite, headach, previous to April 19th, 1850, on which day he passed some blood by the urethra. Dr. Osbrey saw him on the 20th, and found him suffering from frequent desire to pass urine, which was scanty, of a dark sanguineous colour, followed by much straining, and with the passage of blood from the urethra, in which canal a glairy mucous discharge was constantly present; the urine on standing deposited a copious sediment of pus, mixed with blood; there was no pain or tenderness over the loins, the bladder, or in the perineum; his aspect was very pallid; tongue clean; pulse 80, weak and thready; appetite very bad; there was slight œdema of the feet and ankles. He had never suffered from scarlatina, nor was there any disease of the heart or lungs. Dr. William Moore examined the urine, and reported it to be acid, retaining its acidity on the third day after being passed; specific gravity 1·016; highly albuminous; very

deficient in urea; pus corpuscles in great quantity; with some mucus, blood, and epithelium[a]. Dr. Osbrey first directed his attention to allay the irritable state of the bladder, which he effected by the use of liquor potassæ, and hyoscyamus, with mucilaginous drinks; under this treatment the irritability of bladder and the discharge of blood ceased; he complained of pain in the right lumbar region on the 30th of April, but which was completely removed by a blister to that part; and the discharge from the urethra ceased the next day. The urine was still highly albuminous, acid, retaining its acidity for fifty hours; urea deficient; pus corpuscles in great quantity.

May 6th. The sedative mixture was continued, and he was given ten drops of the tincture of the hydro-chlorate of iron three times a day. Next day he complained of headach and sickness of the stomach, but which being temporary, no change was made in his medicine.

May 13th. Urine passed in greater quantity; specific gravity 1·008, albuminous, still deficient in urea; *no pus;* vibriones in great numbers. The iron was continued, and pills of quina, with extract of gentian, two grains of each, given three times a day.

May 30th. Dr. Moore reports the urine to be acid; specific gravity 1·017, no albumen; no pus; urea in fair proportion; vibriones still numerous. He continued the use of the iron until July, 1850, when he appeared perfectly well: urine quite healthy; and he has had no return of the disease up to the present time, now nearly two years.

[a] Dr. Moore states that this must have been basement epithelium, as if there were casts of the tubes, he would have set it down in his register as tubular epithelium. I mention this, as in cases of pus in the urine, the form of the pus globules will often assist in determining whether the pus is derived from the substance of the kidney and the uriniferous tubes, or from the mucous membrane lining its calyces and pelvis, or that of the bladder and urinary canals; if derived from the tubuli, the pus globules will appear entangled in the fibrinous casts, and in the form of purulent casts of the renal tubes, but if from the mucous tissues, the pus appears merely mixed with the basement epithelium.

This case is highly important from the complication of albuminous urine with a copious deposit of pus, and hence involving great nicety in the diagnosis as to the primary seat of the disease; for though the urine was highly albuminous, yet the presence of pus in that fluid made Dr. Osbrey hesitate before deciding as to the true nature of the case; he knew that the fluid in which pus globules float contains albumen, and this, mixing with the urine, renders it albuminous, so that he might expect such a combination of pus with albumen in inflammation of the mucous membrane lining the pelvis of the kidney and the urinary tubes, or in inflammations of the lower portions of the urinary mucous membrane[a]. Yet in this case, though it was evident that irritation of the bladder existed, indicated by the frequent desire to make water, with severe straining and glairy discharge from the urethra,—that pyelitis existed also was probable from the great quantity of pus in the urine, (a rare occurrence in mere inflammation of the bladder), and from its continuing to be passed in large quantities, even after all irritation of the bladder had ceased, as Dr. Osbrey examined the amount daily in a graduated glass measure; yet the fact of the disease having commenced with the symptoms of dyspepsia, the pallid aspect of the patient, the great quantity of pus in the urine, the amount of albumen, too great to be wholly derived from the pus, the deficiency of urea,—evidently not the result of its conversion into carbonate of ammonia, as proved by the urine being passed acid, and[b] re-

[a] Dr. Rees states: "In any case in which the albuminous urine contains a deposit of pus, we must rather look to the mucous tissues of the kidney, and the membrane lining the bladder and urinary canal."—*Medical Gazette*, July, 1851.

[b] Mr. Hamilton (in a valuable communication on pus in the urine, published in this Journal for May, 1851) has called particular attention to the fact of urine "remaining acid after having stood many hours, in cases in which pus was present in the urine, but not secreted by the mucous membrane of the bladder;" and has contrasted these with "cases of inflammation of the bladder in which pus, secreted by the visceral mucous membrane, existed in the urine, and in which that fluid speedily became alkaline, the result of decomposition."

maining acid after standing for three days,—all these circumstances, added to the presence of œdema of the feet, made Dr. Osbrey give his opinion that it was a case of pyelitis, secondary to a diseased condition of the blood, which had induced cachectic nephritis; he was confirmed in his opinion by the persistence of albumen in the urine after all trace of pus had disappeared from that fluid. The successful result of the general treatment by iron proved in the most satisfactory and convincing manner that he was correct.

Although in all these cases the use of iron was commenced at an early period of the disease, and even in some of them when there must have existed congestion of the kidney, evidenced by blood in the urine, and pain in the loins, yet I do not recommend its use indiscriminately; and I wish to impress on my readers that they were all cases of what we may term passive congestions, not accompanied by any fever, and differing totally in character from those cases of albuminuria we meet with in acute inflammatory dropsy caused by exposure to cold, or succeeding to scarlatina,—for in such cases we must not venture to give iron until we have first subdued all inflammatory tendencies by antiphlogistic treatment, and excited the action of the skin and bowels. Indeed in every case when we give iron we must be especially careful to keep up a regular action of the bowels; and if there be dropsy we must relieve the blood of the excess of water contained in it by active purging with compound powder of jalap, and small doses of elaterium.

The previous history and symptoms actually present in the cases detailed in this communication, as well as the morbid appearances which I have met with in similar cases, in which I had an opportunity of examining the state of the organs after death, convince me that the structural changes in the majority of cases ranked under the denomination of "Bright's disease" are very different from those resulting from any form of ordinary inflammation:—I therefore coincide en-

tirely with Dr. Handfield Jones, that "the textural changes, as they have been traced in the kidney, seem to result from a slow gradual change occurring in the renal tissue proper, and the essential cause of this morbid change is the supply of unhealthy blood plasma to the gland; herewith a general deterioration takes place of all the most important textures; there is but little, seldom any, trace of unequivocal products of inflammation, certainly they are not the prime cause of the morbid change, and neither subacute inflammation of the kidney, nor chronic desquamative nephritis, conveys a just idea of the pathological condition which really constitutes Bright's disease"[a].

The following case is illustrative of the beneficial effects of iron in the more advanced stages of the disease.

Case IV.[b]—Catherine M'Kenna, aged twenty-one years, of a pallid, emaciated appearance, was admitted into the Meath Hospital, March 17, 1852. She complained for the first time, about nine months since, of pain and tenderness in the epigastrium and abdomen; after a short time the abdomen began to swell, and has continued gradually to increase; the pain and tenderness were accompanied by a dry retching, which she states used to attack her every day about 1 o'clock; this dry retching continued for about three months, but at no time had she any regular vomiting; her abdomen is now much distended, but chiefly with air; there is some ascites, and also enlargement of the right lobe of the liver; her appetite is very bad, and she suffers from thirst; she complains occasionally of headach; and has a short, dry cough, with perspirations at night; there is a shade of dulness with increased resonance of voice under the right clavicle, but no rale; she passes a large quantity of urine of a very pale colour and alkaline re-action, specific gravity 1·004; no precipitate is caused by heat, but on adding nitric acid a copious precipitate of albumen is formed[c];

[a] London Journal of Medicine, May, 1852. [b] Reported by Mr. Daniel Ryan.

[c] Dr. Simpson, Lecturer on Chemistry, tested it most carefully, but could not dis-

no urea can be detected in the urine[a]; there is a slight, cloudy sediment composed of renal epithelium, with casts of the tubes dotted with oil globules. She states that two years since she fell down stairs and hurt her back, but paid little attention to it until some months ago, when the pain returned, but was removed by a blister. She has had leucorrhœa for the last two years; she has never had any swelling of the legs or feet; she is not pregnant; her bowels are always inclined to be too free. I put her first on the use of the liquor ferri pernitratis, in doses of ten drops, three times a day, and afterwards she took ten grains of the ammonio-citrate of iron, three times daily; under this treatment her general symptoms and appetite improved; the urine became acid, but still continued albuminous, though less so than at first, and its specific gravity increased to 1·012; she left the hospital, as she felt much relieved and was anxious to return to her home.

This case is also interesting as affording an example of that form of degeneration of the kidney termed stearosis by Gluge, and corresponding with the fatty degeneration described by Dr. G. Johnson, which in this country certainly appears to be the rarest form, at least among the lower orders. The slight amount of dropsy is also peculiar, as there was no anasarca, and only slight ascites; but I have seen four cases, and similar ones have been recorded by Drs. Bright, Rees, and others, in which convulsions and death have occurred, without the slightest trace of dropsy, and yet the kidneys have presented all the appearances of advanced degeneration. The complete absence of urea from the urine is also interesting, when we consider there were no symptoms referrible to its detention in the blood; and though many persons still attribute the dangerous secondary complications, particularly the occurrence of

cover any traces of urea in the urine. I was assisted in the microscopical investigations of all the cases by Mr. Thomas Ledwich, and the chemical analyses were made by Dr. Simpson.

[a] British and Foreign Review, April 1852, p. 308.

cerebral symptoms, to the presence of an excess of urea in the blood, yet many cases are recorded in which that substance was obtained in considerable quantity from the blood, and yet the patients never presented any symptoms of uremia. Dr. Frerichs offers a theory on this subject which appears worth consideration; he says: "The symptoms of uremic intoxication arise in consequence of the urea accumulated in the blood being converted, by the agency of a suitable ferment, into carbonate of ammonia while yet within the vessels." And he has performed two series of experiments in support of his theory: first by injecting a solution of urea into the veins of animals from whom the kidneys had been removed, and yet they remained free from convulsions for some hours, until ammonia could be detected in the expired air, then convulsions or coma supervened, and after death, ammonia, in large quantity, was found in the blood. In the second series of experiments he injected a solution of carbonate of ammonia into the veins of animals, when convulsions instantly supervened. So that he considers some agent must convert urea into carbonate of ammonia, but what this agent is we only know from its effects: we have no other proof of its existence.

Dr. Bence Jones[a] considers it "most probable that other substances, far more pernicious than carbonate of ammonia, may be produced in the blood in consequence of the insufficient action of the kidneys:" and believes that "oxalic acid or carbonic oxide might accumulate in the blood instead of carbonic acid."

[a] Medical Times and Gazette, May 8, 1852.

Art. V.—*On the Topical Treatment of Acute Inflammation of the Larynx and Trachea.* By Eben Watson, A. M., M. D., Fellow of the Faculty of Physicians and Surgeons of Glasgow, and Professor of the Institutes of Medicine in Anderson's University[a].

It is the almost necessary fate of a new remedy or plan of treatment to be employed at first without due discrimination. Let but its value in one set of cases be established, or supposed to be established, and it will forthwith be employed in many others with the highest expectations, and too readily proclaimed appropriate and useful in the latter as well as in the former. But ere long an enlarged experience shows that such a mode of proceeding was unwarranted, that the remedy fails in some, perhaps in many, cases for which it had been declared an infallible cure, and thus too often it becomes despised and set aside, although in reality efficacious within a more limited range than its injudicious advocates had at first pretended.

Now I cannot help thinking that the "topical medication" of the larynx, as its vaunters delight to call it, runs a great chance of sharing some such fate as that to which I have just referred. Nay, I believe that many excellent physicians are deterred from giving even a trial to this kind of treatment in *any* case, by the apparently equal and always perfect success which seems to have followed its use by certain parties, in diseases the most dissimilar and varied, if only the larynx or the pharynx could be in any way supposed to be implicated therein. I am the more anxious, therefore, having for some years past practised the topical treatment of the larynx pretty extensively, and having found it highly efficacious *in certain kinds* of laryngeal disease, to contribute my mite of experience towards the establishment of its therapeutic value, as well as to defend it from the great danger of an indiscriminate laudation.

[a] This paper was read to the Medico-Chirurgical Society of Glasgow on the 13th of April, 1852.

In the first place, then, I shall begin by endeavouring to point out the *modus operandi* of a solution of the nitrate of silver when applied to an inflamed mucous membrane, and if I shall at all succeed in this part of my subject,—if I shall be able to demonstrate the therapeutic indications which such a solution is fitted to serve,—a due limitation of its applicability will follow as a necessary consequence, and a settled position will have been gained for the topical treatment of the larynx, among the remedial agents of a rational medicine. For although it would be rash in the present state of science to expunge from the *materia medica* that numerous class of medicines of whose action we can give no satisfactory explanation, and for the introduction of which we are solely indebted to empiricism,—that is, to an imperfect induction of isolated facts,—yet, undoubtedly, a much higher place in our confidence should be acceded to those remedies which can be shown to act according to some one of the ascertained laws of the animal economy, and which are, therefore, based on the sure foundation of physiological science.

There is a little experiment, simple, and easily repeated, which is familiar to all who have paid attention to the recent advances made in our knowledge of the inflammatory process, and which presents us with an excellent occasion for observing the action of the solution of nitrate of silver, in the different stages and degrees of that morbid state. I refer to the excitement of inflammation in the web of a frog's foot stretched out under the microscope. When, for example, a red-hot needle is passed through the web, the following are the phenomena observed:—A spot in the centre of the inflamed part is sphacelated, destroyed by the passage of the needle through it; a circle round the spot is usually found in a state of complete congestion, the vessels being dilated, and the corpuscles almost perfectly stationary within them, and in the part beyond this circle the vessels are not so much dilated, and the

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stasis of their contents is not so complete. The stream is seen passing slowly away into the collateral circulation of the unaffected parts of the web.

Now these two circles represent two degrees of inflammation, which it is important to distinguish wherever they occur, and, perhaps, especially when the seat of morbid action is the mucous membrane of the larynx or trachea. That part of the web of the frog's foot in which the stasis was complete represents the most intense, or sthenic degree; the other, in which the stasis was not so complete, represents what is usually called the sub-acute, and perhaps chronic, varieties. And the effects of the solution of caustic on each of these parts is markedly and importantly different. In the part which is most intensely inflamed, the solution, in the direct ratio of its strength, increases the stasis of the blood within the vessels. The latter seem to be unable to dilate further, and are, therefore, little changed, but the nitrate of silver acts through the coats upon the blood which they contain by causing its partial coagulation, and, likewise, by withdrawing water from the serum for the crystals of the nitrate which begin partially to form if the solution is strong. In that part of the web, on the other hand, which had been less intensely inflamed, the stimulant solution causes a renewed and increased dilatation of the blood-vessels, and the retarded current moves on in them more freely than before; a cure being thus speedily effected if the exciting cause of the inflammation has ceased to act.

That precisely similar degrees of inflammation occur in the mucous membrane of the larynx and trachea with those just described as artificially produced in the frog's foot, I need hardly attempt to prove, for it will be at once admitted that there are three kinds of acute laryngitis: one in which no false membrane is formed; a second, in which a false membrane is formed, but in which the pharynx, as well as the larynx, is affected, viz., the diphtherite of French writers; and, lastly,

that intense local inflammation of the larynx and trachea, accompanied by exudation, to which in this country we generally restrict the term "croup."

1. The first kind varies in its intensity from the most trivial catarrhal irritation to a severe inflammation of the whole thickness of the mucous membrane lining the windpipe. It very often commences about the fauces and passes downwards, causing cough more or less croupy in its character, difficulty of breathing, and hoarseness of voice. It is accompanied by fever of greater or less intensity, and its chief physical signs are, increased loudness of the respiratory sounds in the larynx and trachea, with occasionally a whistling sound, from the absence of the natural mucus of the part, and from partial spasm of the glottis. This kind of laryngitis is more frequently complicated with bronchial inflammation than with pneumonia, and as the patient sinks from non-aëration of the blood, the most prominent symptoms of the laryngeal affection gradually give way, and it is often difficult after death to detect more than a slight redness of the bronchial mucous membrane. The inflammation sometimes, however, terminates in œdema glottidis, and but rarely in the exudation of a false membrane.

I have mentioned the chief features of this kind of laryngitis, first, because it is very common in its slighter degree, and is by no means rare, even in its utmost severity; and, secondly, because it is not usually a very sthenic disease, or, to speak more correctly, it does not present so very high a type of inflammation as that which is characterized by lymphatic effusion, and is, therefore, an example of that degree of this morbid process, comparable to the outside circle in the inflamed portion of the frog's foot. The proof of this lies in the absence of exudation,—the usual complication being bronchitis, not pneumonia,—the longer duration of the disease, and its frequent termination in serous effusion into the glottis, or by passing into the chronic form of laryngitis.

It will be expected, then, that the solution of caustic

should act well in such cases, and so it does: but perhaps in none is a greater amount of discrimination necessary in the adaptation of the strength of the solution to the severity of the inflammation which may be present, as well as in the choice of the proper time for commencing the topical measures. For it must not be thought that I advocate restriction to any one remedy, local or general, in the treatment of this disease. In the severer forms of the affection especially, depletion of some kind will at first be necessary to check the violence of the inflammation, and an emetic will be useful in restoring the moisture of the mucous surface. It is after the use of both these remedies that the topical application is alone admissible, nor can it even then be employed to the exclusion of other means. In children it will be especially necessary to repeat the emetic several times during the progress of the case, and in very few will the judicious practitioner refrain from the use of counter-irritation to the outside of the throat while he is proceeding with the topical treatment internally. It is, however, with the latter that I have specially to do at present, and therefore to the management of it I shall, in a great measure, confine my remarks.

Contrary to what might, *a priori*, be expected, the result of those experiments I before alluded to is, that the more intense the degree of inflammation of the laryngeal lining, the weaker ought to be the solution of the nitrate of silver applied to it. In those cases in which the intensity of the inflammation has never been great, or in which, as is more likely to happen, the primary violence of the disease has been subdued by other treatment, a stronger solution may be used with advantage. Its first effect, when thus judiciously applied, will be to coagulate the albuminous film upon the surface of the membrane which had been stripped of its epithelium, and thus to cover and protect it. Another, and almost simultaneous effect, is to stimulate the basement membrane to form new epithelium, and to secrete new mucus, and thus the artificial film

of coagulated albumen is by-and-by replaced by a more natural covering, and the surface is lubricated by its appropriate moisture. If, then, a renewal of the morbid process could be prevented, a cure would already have been accomplished, but this is seldom or never the case. The good effects of the topical application wear off in a few hours, and the former abnormal phenomena may even in that time have re-appeared in nearly equal severity. The treatment must therefore be continued, —the touching of the larynx must be repeated frequently for some days, and indeed until all the symptoms of laryngitis have completely disappeared.

In some severe cases of this kind, especially in adults, there is great danger of a chronic thickening of the laryngeal mucous membrane being left behind, and of the voice being thus permanently impaired. It may, therefore, in such cases be a very good precaution to give a little mercury towards the end of the acute attack; but whenever the mouth begins to be affected, the topical application of solution of caustic must be stopped; for the laryngeal lining, especially at its upper part, is then far too vascular and irritable to bear the touching, and its only effect would be to cause still greater excitement of the part. It is for this reason chiefly, and because I put considerable faith in the topical remedy, that I should recommend the mercurialization to be postponed until the termination of the acute stage. After the mercury has had its required effect, it is often a good plan to repeat the topical treatment for a time, when its efficiency will be found very great in restoring the tone of the voice, and fitting the patient to bear a change of his apartment in the first place, and, ere long, removal to the country.

2. Regarding the second kind of laryngitis, viz., that which forms part of the diphtherite of Bretonneau, I have little to say, not having met with the affection in practice. It seems generally to be connected with an atonic state of the general system; the whole fauces, the gullet, and the stomach itself, are

often affected at the same time, and in the same way, as the lining of the air-tubes. The exudation, which in these cases occurs after a short stage of intense erysipelatous redness, consists almost entirely of albumen, and covers the whole of the parts affected, sometimes in considerable thickness. It is, therefore, in many respects different from the more local, firmly adhering, lymphatic exudation of true croup.

This seems to be a common affection on the Continent, especially in Paris, and also in New York according to Dr. Horace Green, who tells us that solution of caustic acts admirably as a topical application in such cases. It seldom, however, occurs in this country, at all events in this city, except as an occasional, and happily rare, form of epidemic.

3. True exudative croup is altogether a different disease. It appears in very sthenic conditions of the general system; the blood is invariably rich in fibrine and corpuscles, and the exudation which forms in the larynx and trachea always contains, and is sometimes chiefly composed of, fibrine. It is, moreover, a purely local affection, seizing at once on the larynx or trachea, and confining its chief violence to one or both of these organs. It is, besides, more frequently complicated with pneumonia than with bronchitis,—another proof that the degree of inflammation present is very intense, and to be compared with that which exists in the web of the frog's foot around the puncture of the red-hot needle. And my experience of the effects of the solution of caustic, in cases of croup, justifies this comparison, and confirms the important inference that such treatment is unsuitable for the degree of inflammation present in them; for I have always found the symptoms of congestion in the laryngeal lining, such as pain and difficulty of breathing, increased by the application of even a weak solution of nitrate of silver, and the very act of applying the solution is hurtful in these cases: the sponge generally brings away part of the false membrane upon it, and leaves the delicate and highly vascular tissue beneath exposed, and often bleeding.

Pain, anxiety, and, I fear, increased exudation, and sometimes ulceration, are thus produced, the original disease being thereby aggravated.

To show that I have not been too easily led to these conclusions, and to give force and point to what I believe to be a very important statement, I shall relate the two following cases, which I venture to think interesting in many points of view, and which I have, therefore, selected from my case-book. One of them illustrates the action of the topical application in an adult case, and the other in that of a child.

Case I.—The subject of this case was a gentleman past the middle period of life, and, before the illness which I am going to describe, particularly strong and healthy.

One evening of the winter before last he was suddenly seized with difficult respiration, tightness in the throat, harsh, dry, whistling cough, and high fever. All the symptoms of croup, indeed, became very soon but too well marked; and, a few hours after the apparent commencement of the attack, the following were the physical signs which presented themselves:—The number of respirations in the minute was much increased, and yet the feeling of oppression on the chest remained unabated, so incomplete was the inflation of the lungs: indeed the respiratory murmur was but feebly heard in the upper parts of the chest, while the bronchial sounds were dry and snoring in their character. In the trachea the inspiration was long, and accompanied by the harsh sound of the air passing along the dry and narrowed tube. A little higher up, and chiefly at the commencement of inspiration, the glottis was heard vibrating so as to occasion a stridulous sound.

When the patient spoke he suffered great pain, and increased feeling of anxiety. His voice was feeble and broken, being at times deeper, and then suddenly much slighter, than his ordinary tone.

There could, therefore, be no doubt that this was an instance of acute tracheal croup, accompanied by exudation. It

was treated as such, by emetics, purgatives, hot baths, bleeding, antimony, and calomel, with a blister on the trachea, and in the evening I commenced to apply a solution of twenty grains of nitrate of silver in an ounce of water to the interior of the affected organ; but each application gave great pain and uneasiness, and increased the sense of suffocation. The violent fits of coughing which were thus produced undoubtedly occasioned the separation of small portions of the false membrane, but that was no improvement, since the surface thus exposed was tender, unprotected, and often bleeding. I next followed Dr. Horace Green's example, and increased the strength of the solution twofold. This, however, only made matters worse, and indeed the patient himself began to dread the repetition of the proceeding. Still my faith in the remedy was not completely exhausted: I determined, before abandoning the topical treatment altogether, to use a weak solution of the nitrate of silver; I therefore diluted it to ten grains, and ultimately to five grains, in one ounce of water, and yet I was unsuccessful. It was, indeed, too apparent to me that the larynx was not in a state to bear either the stimulant solution, or the presence, for however short a time, of the sponge by which it was applied; I therefore gave up the topical treatment entirely at this time, and used more ordinary measures. The patient was still further depleted, and more decidedly mercurialized. He was likewise frequently blistered during the next month, by the end of which time he was much improved, but still had a good deal of hard, whistling cough, dyspnœa when he moved about, and great pain when he spoke, referred to the glottidean region. The tone of voice was weak, but not unusually hoarse.

The most careful examination of the chest still showed that the lungs were free from disease. The respiratory sounds in the trachea were loud, harsh, and dry, and were accompanied by a pretty constant râle, as if there were one or more valvules of exudation matter still adhering to the walls of the trachea. The vibration of the glottis in breathing and cough-

ing was not so free as formerly, indicating a degree of œdema of the organ.

On opening the mouth, the fauces were seen to be red and swollen, and the epiglottis was felt by the finger covered with soft and doughy mucous membrane.

After careful consideration of all these circumstances, it was determined, in consultation with my father, that two caustic issues should be opened, one on each side of the thyroid cartilage; that the iodide of potassium should be administered in decoction of sarsaparilla; and that I should again apply the caustic solution to the interior of the larynx and trachea, now that the inflammation had passed the acute stage. Under this plan of treatment the patient made daily advances towards health, and was soon able to take exercise out of doors, wearing a respirator.

The effects of the topical treatment during this latter period were as manifestly beneficial as formerly they had been hurtful. The strength of the solution was at first only ten grains to the ounce of water, but was gradually increased to a scruple in the same quantity. After each application the patient found that in a short time his breathing was freer, his cough less frequent, and his voice stronger; but this improvement at first lasted only about forty-eight hours, at the end of which period the application was always renewed with the good effect of sustaining the improvement. By-and-by, however, the intervals were lengthened with impunity; the gentleman spent the summer at the coast, and is now perfectly well.

I think it worth mentioning, in conclusion, that I still see this gentleman occasionally for a feeling of dryness in the throat, which nothing but the stimulant application seems to relieve. This appears to be a very common state of matters after the caustic solution has been applied to the throat for any length of time. Hence I now and again see a number of my old laryngeal patients who have been cured of all their symptoms with the exception of this feeling of dryness, and it is

often both intense and annoying. I believe the most of them would disregard it were it not for the fear of a relapse into their former state, and I therefore encourage them to forget it if possible, and to use such means as rubefacient liniments externally, or some simple gargle. Still it must be confessed that no remedy for this disagreeable feeling is so effectual as the solution of caustic, and if it be not too strong, and if the intervals of its application be not injudiciously short, I do not think its continued use in the cautious manner just indicated will do any injury to the mucous membrane.

But to return from this digression;—the case which I have narrated proves, as clearly as any single case can prove, that the topical treatment is unsuitable during the acute stage of exudative croup: and, were it not for the inconvenient length to which it would protract my paper, I could relate many others, the subjects of which were children, and which all go to corroborate the above conclusion. I have in these cases invariably seen good reason to stop the topical treatment if I had begun to use it early in the disease, because I found that it retarded, if it did not prevent, their favourable progress. One of the most marked of the cases to which I allude was the following, and I relate its chief features here because it is the last case of acute exudative croup in which I have used, or intend to use, the solution of caustic.

Case II.—The patient was a girl, four or five years old, attended by my friend, Dr. Peter Stewart of Eglinton-street.

This little patient was suddenly seized with symptoms of acute croup, about the middle of last winter. Dr. Stewart was immediately called in, and at once instituted the most judicious measures to check, if possible, the untoward progress of the malady. Among other things he applied a solution of caustic to the pharynx and upper part of the larynx.

Unfortunately, however, as sometimes will and must happen, under the best treatment, the patient's state rapidly became worse, and Dr. Stewart requested a consultation with

my father; and a doubt occurring to the former that possibly he might not have passed the probang fairly into the larynx, he likewise asked me to see his patient and apply the solution of caustic for him.

The child had been about forty hours ill when I saw her, and was evidently in a most dangerous condition. The exudation was very abundant in the trachea, as evinced by the sharpness of the inspiratory sound heard over it, and by the faintness of the vesicular murmur in the lungs; the glottis, however, vibrated during coughing and speaking; and was, therefore, free of œdematous swelling. The pulse was quick, but not feeble; the surface of the body was hot and moist; and the face was of a dingy hue, the lips being almost livid.

I introduced the probang, the sponge of which had been moistened with a solution of twenty grains of caustic in an ounce of water, so easily through the rima glottidis, that I feel quite convinced that Dr. Stewart, who is in the daily habit of using this plan of treatment in many other cases, especially in hooping-cough[a], had likewise reached the seat of disease, and that there had been a fair trial of the topical treatment in this case from the commencement. I repeated the application thrice during my first visit, and Dr. Stewart renewed it again

[a] This mode of treating hooping-cough was originally proposed by myself in the Edinburgh Monthly Journal of Medical Science for 1849, and is slowly, but, I trust, surely, gaining confidence both with the profession and with the public. For the explanation of the theory of this mode of treating hooping-cough, I refer to my original paper; but I hope the following numerical results of the practice will not be uninteresting to the readers of the Dublin Quarterly. M. Joubert, of Cherion, has used the topical treatment in sixty-eight cases of hooping-cough, with the following results (see the Bulletin de Thérapeutique for January, 1852; and also the Edinburgh Monthly Journal for May, 1852, page 257):—

A speedy cure resulted in	40	Cases.
Great relief, and shortening of the disease, occurred in	20	,,
No change was effected in	8	,,
Total, . . .	68	

The cases of this disease which I have myself treated, from beginning to end, in

in the evening. At my second visit, next morning, I used a stronger solution, viz., one of forty grains to the ounce of water. After each application the child seemed a little easier, perhaps from the passage being partially cleared by the sponge and by the child's own efforts, but she always became worse in a very short time; and although all the ordinary means had been carefully used during the whole progress of the case, besides the topical measure, still the child's state was evidently becoming very hopeless. The pulse was more rapid, but not as feeble, as might have been expected, and the colour of the skin was more dingy, indeed it was almost livid. The child died that evening, and I regret to add, that no inspection of the body was permitted.

In this case the failure of the topical treatment was far too marked to occur in any one's practice without exciting very serious reflections regarding its employment in the disease of which it was an example, and it led me to look back to my notes of other similar cases, as well as to institute some such experiments as that which I formerly narrated. The results of these observations and reflections have been to convince me of the total unsuitableness of the treatment in question to acute cases of exudative croup.

But I may here be met with the objection that if, in my

this way amount at present to 57, in all of which a more or less speedy cure was effected, as the following statement will show :—

38	Cases were cured in from	10 to 14 days.
19	„	„	3 to 4 weeks.
57			

Combining these two Tables, we have, in the first place, 125 cases of hooping-cough treated in this manner without one death. Only 8 out of the whole number resisted the treatment; of the rest, 78 were speedily cured, and 39 were greatly relieved and shortened. What better proof can be asked of the efficacy of the topical treatment of hooping-cough; and what now prevents its general adoption by the profession? Am I not warranted in believing that were it so adopted much suffering would be saved and many lives would be prolonged beyond the first, the most interesting, but perhaps also the most dangerous, epoch of human existence?

cases of croup, the topical method was unsuccessful, a very different result ensued in Dr. Horace Green's cases. This impression, however, will not, on examination, be found to be so correct as many may be inclined to think. Nor is it, in my opinion, detracting from Dr. Green's merit to hold that it consists in having effectually directed attention to the general subject of topical applications to the interior of the larynx rather than in recommending that treatment in cases of croup.

Dr. Horace Green illustrates his little work on Croup by thirteen cases. He may possibly refer to others throughout the work, but these are the only examples fully related, so that they can be judged of independently by the reader; hence they are carefully numbered so as to permit of easy reference. Of these thirteen cases, two are quoted from Mr. Ryland's work on the Larynx, chiefly for the sake of the account given by that author of the morbid appearances after death. In these, of course, the topical treatment was not used, so that the cases which illustrate this treatment given by Dr. Green are reduced to eleven. Nor am I convinced that these were all cases of true exudative croup; nay, I think it is certain they were not; for No. v. was a mere hoarseness, and No. viii. was a spasmodic affection of the glottis which came and went without any symptom of croup at all. Nos. ii., vii., and x., were apparently cases of acute œdema glottidis: leaving only six cases the symptoms of which resemble those of croup. Even some of these six have more the characters of diphtheritis than of croup, and in one of them (No. xiii.) the affection followed measles. In only four of the six cases was the disease fully developed, and of them one-half died. But, supposing that all the eleven cases related in this book were really cases of croup, more or less severe, I do not think that the mortality among them, viz., three deaths in eleven cases, was less than it generally is in the ordinary run of croupy cases occurring in the better ranks of life, and treated in the usual way; and therefore it follows that Dr. Green's experience, so far as we have it in his work on Croup,

does not show that his success in the treatment of cases of that disease was increased by his using the topical applications to the interior of the larynx; for he very properly used other measures as well, and the result has been a mortality not at all less than if he had neglected the topical treatment altogether. I consider it no small corroboration of my opinion, in regard of this point, that M. Trousseau states in the "Union Médicale" for 1851, No. 100, as one reason of his superior success of late years in the treatment of severe cases of croup, that he has discontinued the application of a strong solution of caustic to the larynx and trachea, which he used formerly to insist upon.

The termination of acute inflammation of the laryngeal mucous membrane, whether that inflammation had been of the simple or of the exudative type, in œdema of its loose subjacent tissue, is an event so remarkable and important that I have reserved until now the few remarks which I wish to make on its topical treatment. I believe that the occurrence of the lesion referred to is by no means infrequent, and that it is always attended with imminent danger to the patient's life. The rapidity with which the inflammatory stage sometimes terminates in this manner is sufficiently remarkable to have struck every one who has observed cases of the kind. In some of these it is the result of constitutional debility, however that may have been produced; while in others it seems referrible to a peculiarity in the nature of the morbid process itself. In the former class of cases it generally arises during the progress of some exhausting disease, such as typhus fever, or towards the end of exudative croup itself when it is always a formidable and often a fatal complication. And even when it occurs as a more primary disease, the inflammation of the mucous membrane appears to be reduced in intensity by the very occurrence of the serous effusion, although it had previously been even of the exudative type. From what has been formerly stated, then, regarding the action of a solution of caustic applied to a subacutely inflamed mucous membrane, it might, *a priori*, be expected to produce a bene-

ficial effect on the œdematous glottis; and this expectation has been remarkably fulfilled in my experience, as the following instance will sufficiently exemplify.

Case III.—A young child, of eight months' old, had severe hemorrhage from the gums after division of them over the incisor teeth, and in the exhausted state which followed, he caught cold, and became affected with the ordinary symptoms of croup, which were chiefly combated by an emetic, counter-irritation over the throat and chest, and by repeated small doses of calomel. But very soon the chief, nay, only symptom became that of impeded respiration. The child's efforts during inspiration, the dry, whistling sound which accompanied it in the trachea, the nearly total absence of vesicular murmur in the lungs, and the short expiratory sounds, taken along with the previous state of the little patient, rendered it evident that œdema glottidis had occurred; and if to this it be added that the pulse was feeble, the patient pale and exhausted, and that he could hardly be made to receive nourishment,—his extreme danger will not be questioned.

I introduced the probang down to the glottis, but not through the rima, owing to the swelling of its margins. The strength of the solution used was thirty grains to the ounce of water, and it was applied three or four times at short intervals. The effect was soon apparent. Some coughing, and the expulsion of tough muco-albuminous matter first followed, and then the child became quiet, the breathing was freer, although, of course, there was still considerable obstruction at the glottis. In a few hours this obstruction seemed to be increasing, and the application of the caustic solution was again renewed in the same way, and with equally favourable results. The calomel was continued, and a warm water enema was administered, after the action of which the child took the breast, and slept for a short time. The future progress of the case was marked by a gradual but steady improvement. The calomel was soon

stopped, the bowels were duly regulated, and the topical applications were persevered in daily for two or three weeks, by the end of which time all obstruction to the breathing, as well as the cough, and even a degree of hoarseness which had latterly been observed, had completely disappeared, and the child's general health rapidly improved.

No example could, I think, better illustrate the efficiency of the topical treatment in cases of œdema glottidis, even when occurring with the most serious concomitants. In the preceding instance the œdema happened during the progress of pretty severe laryngeal inflammation, and in a very debilitated state of the system from loss of blood. Yet the application of a caustic solution had always a marked and beneficial effect on the state of the diseased organ, and mainly contributed to the successful issue of the case. Nor can it be wondered at that two or three weeks elapsed before all the laryngeal symptoms had completely disappeared, for the child required that time to make up the blood he had lost, so as to enable the larynx to resume its healthy tone. But in those cases in which œdema glottidis is a more primary and uncomplicated disease, the good effects of the topical treatment are very much more speedily seen and felt. Indeed, as it is then a purely local affection, the means employed for its cure may, in many cases, be purely local too; for instance, as in the following example.

Case IV.—About the middle of last winter I was suddenly summoned to visit an infant, only two months old, that had been ill with a rough cough for some days, but as the child's health was little affected, she was thought to be only labouring under a slight passing cold: however, on the morning of my visit, the child was found to have great difficulty in breathing, and when I saw her that symptom had become so very severe, that the patient's face was nearly livid, and she was tossing about on the nurse's knee almost convulsively.

The physical signs were those of swelling and nearly com-

plete closure of the glottis. The infant's cry was almost extinguished, being reduced to a faint whistling sound, and the respiratory murmur could scarcely be heard in the chest.

I immediately touched the glottis with a strong solution of caustic, with the instantaneous effect of assisting in the evacuation of a considerable quantity of ropy mucus, and in relieving the obstruction to the respiration. A purgative enema and a tepid bath were administered, and in a few hours the touching was repeated. The breathing was now much improved, and the patient passed a good night. Only four or five repetitions of the topical application were necessary in this case, and no other treatment was employed except regulation of the bowels and confinement to the house. The child was perfectly well in a few days.

On reviewing the whole subject, then, the following are the principal conclusions to which my observations, experimental and clinical, have conducted me:—

1st. The solution of the nitrate of silver, when applied to an inflamed mucous membrane, acts differently, according to the intensity of the inflammation that may be present; in the asthenic varieties it operates as a stimulant of the capillaries of the part, and likewise of its secreting apparatus, while in the sthenic variety it increases the congestion of the membrane, chiefly by diminishing the fluidity of the blood in its vessels.

2nd. In acute laryngitis in which there is no false membrane, and probably in diphtheritis in which there is an albuminous exudation, the local application of solution of caustic, varying in strength inversely in proportion to the intensity of the inflammation, may be employed with more or less speedy benefit.

3rd. During the violence of true exudative croup the stimulant application to the part affected is injurious, but when the disease begins to yield to antiphlogistic and other treatment, it may assist in the cure.

4th. Œdema glottidis, whether occurring as a primary dis-

case, or as a complication of other morbid states, is always speedily relieved, and in some cases effectually cured, by the application of strong solutions of the nitrate of silver to the œdematous organ.

And 5th. It follows as a corollary, derived partly from the foregoing conclusions, and partly from the results of my experience of the topical treatment communicated to this Journal in November, 1850, that the solution of caustic acts beneficially in only one, viz., in the asthenic variety of laryngeal inflammation: for it matters not whether such has been the original character of the affection (acute but asthenic cases), or whether it has become so under the combined influence of time and general treatment (chronic cases).

Art. VI.—*Observations on Hermaphroditism, illustrated by a remarkable Case.* By Awly P. Banon, M. D., Fellow of the Royal College of Physicians, Edinburgh, and of the Royal College of Surgeons, Ireland; Physician to the Richmond and Grangegorman Government Prisons, and Acting Physician to the City of Dublin Prisons; Surgeon to Jervis-street Hospital, &c.

Although true hermaphroditism, or a condition presenting the characters of both sexes in the same individual, and each capable of separately discharging its functions, has not been met with in man, or even amongst the higher classes of animals, it is, nevertheless, of the deepest interest to consider those departures from a normal development of the generative organs, in some instances approaching the true hermaphroditic state, which are occasionally to be met with, and of which a very remarkable instance recently came under my notice.

The importance to individuals of this class of a thorough acquaintance with the subject on our part, especially in being capable of discriminating cases of what are termed "spurious hermaphroditism," must be admitted, when we recollect the

doubts which so often arise as to the predominant sex of the parties concerned, and the mistakes, of not infrequent occurrence, by which they may be doomed through life to submit to the education and habits of the sex not truly their own. Thus, in describing a case of hypospadic malformation in the male[a], Otto gives the following curious particulars:—"The person had lived ten years in a state of wedlock with three different men; but at the age of thirty-five, an action of divorce was brought against her by her third husband, accusing her of being affected with some disease of the sexual parts that rendered the performance of the connubial act on his part extremely difficult and painful. After some difference of opinion between the two medical men, to whose professional examination the wife was submitted, it was at last considered that she was in reality a male: and the case came then under the investigation of the members of the Royal Medical College of Silesia, who confirmed this opinion. The imperforated penis was an inch and a half in length, the perineal fissure forming the false vagina was, at the posterior part of its orifice, bounded by a distinct frænulum, but was of a size sufficient to receive the glans of the husband for an inch and a half in depth. At the bottom of this fissure could be seen the orifice of the urethra, with the ducts of Cowper's glands opening on its surface. The right half of the scrotum contained a small testicle, about the size of that of a boy of ten years of age; the left testicle lay likewise external to the abdominal ring, and was still softer and smaller than the right. Both were furnished with spermatic cords. The general configuration of the individual was strong, muscular, and meagre; the beard was thin and soft, and the face, mammæ, thorax, pelvis, and extremities, were evidently masculine."

Amongst other remarkable instances I may also mention that detailed by Julien and Soules[b].

[a] Cyclopædia of Anatomy and Physiology, vol. ii. p. 693.

[b] Observations sur l' Histoire Naturelle, sur la Physique, et sur la Peinture, tom. i. p. 18.

" Maria Nonzia was born in Corsica, in 1695, was twice married as a female, and at last divorced, in 1739, by her second husband, after having lived sixteen years in wedlock. The penis was two inches in length, but imperforate, and the meatus urinarius was placed at its root. Two bodies like ordinary sized testicles, and furnished with spermatic cords, were felt in the divided scrotum, and there was a narrow false vagina or perineal canal, one inch and three lines in depth. The character and appearance of the person were masculine: the visage was bearded; the mammæ were as fully developed as in the adult woman, but the nipples were each surrounded with hair."

The cases here related sufficiently point out the important light in which the study of these malformations of the sexual organs, and the means by which they may be discriminated, should be viewed; and it may be well in this place to give a short description of those deviations from the natural formation of the external organs of generation most usually to be met with, and to which the term " spurious hemaphroditism" is applied. At the same time it must be acknowledged that many cases have occurred in which it has been found extremely difficult to point out the predominant sex, and in several instances the most scientific and careful observers have been completely at fault; and it may also be remarked that some of those cases, considered during life to be merely malformed as to their external organs, have been found, on dissection after death, to possess the internal reproductive organs of both sexes, in a more or less perfect state.

From the most recent researches of physiologists it appears to be well ascertained, that in the embryo both sides of the body are primarily formed, and exist independently of each other, with a strong tendency, however, as they advance in development, to a union in the centre or median line. Now, instances are not uncommon of a departure from this natural process, or a non-completion of the union of the symmetrical

halves of the body, in the spine and elsewhere; and amongst them I may mention what is termed "hypospadias" in the male, constituting one of the most common forms of "spurious hermaphroditism." This malformation, known also by the name of "cleft scrotum," may exist in a greater or lesser degree, but is manifestly a result of an imperfection of that process of union above alluded to, which when complete is marked at the perineum by the "raphé." In some individuals the imperfection is so slight as not to prevent the power of procreation, cases having occurred in which an hereditary disposition to hypospadias was observed, it being present in both father and son, and here, of course, no difficulty could occur in determining the sex; but in other instances, accompanied by a non-descent of the testicles, and by an imperforate state of the penis, the deformity is so great that the cleft scrotum bears a strong resemblance to the female parts of generation, the opening of the urethra in the perineum having much the same relation to the penis as it has in the female to the clitoris, the similarity being rendered still more striking by the perineal fissure forming a false vagina, as in the case described by Otto, "capable of admitting the glans of the husband for an inch and a half in depth."

The next form of spurious hermaphroditism which it is important to mention arises from an increased or unnatural development of the female clitoris, for an explanation of which it is also necessary to refer to the mode of growth in early fœtal life. Previously to the fourth month the clitoris does not bear the same relation to the other parts of generation as is afterwards observed; it is, up to that period, as large and well developed as the penis in the male, but an arrest of growth then takes place by which the proportion it bears to the other parts of generation in after life becomes very much altered. Exceptional cases, however, occasionally occur, particularly in warm climates, in which such arrest of development does not take place, but, on the contrary, the clitoris attains to a very consi-

derable magnitude, closely resembling the male organ, the usual dimensions of which it even exceeded in the following case, given by Beclard[a].

" Marie Madeline Lefort was at that time aged sixteen years. The proportion of the trunk and members, and of the shoulders and pelvis, and the conformation and dimensions of this last part of the body, were all masculine; the volume of the larynx also, and the tone of the voice, were those of an adolescent male; a beard was appearing on the upper lip, chin, and region of the parotids; some hairs were growing in the areola round the nipple; and the mammæ were of a moderate size. The inferior extremities were furnished with an abundance of long, hard hairs; the symphysis pubis was elongated as in man; the mons veneris rounded, and the labiæ externæ were covered with hair. The clitoris was ten and a half inches (?) (twenty-seven centimetres) in length when at rest, but somewhat more when erect; its glans was imperforate and covered in three-fourths of its circumference with a mobile prepuce. The body of this enlarged clitoris was furnished inferiorly with an imperfect canal, which produced a depression in it, instead of that prominence of this part which exists in the male penis. This canal was pierced along its under surface and median line by five small holes capable of admitting a small stylet; and one or more similar apertures seemed to exist in it after it had reached backwards within the vagina. The labia were narrow and short, and the vulva or sulcus between them was superficial, being blocked up by a dense membrane, which, under the pressure of the finger, felt as if stretched towards the anus over a cavity. At its anterior part, or below the clitoris, there was an opening capable of admitting a sound of a moderate size, and this sound could be made to pass backwards behind the membrane closing the vulva, which, when felt between the point of the instrument and the finger, seemed about twice as

[a] Bulletin de la Faculté, 1815, p. 273; and Cyclopædia of Anatomy and Physiology, vol. ii. p. 688.

thick as the skin. The urine was passed by this opening, and also, according to the report of the individual herself, through the cribriform holes in the canal extending along the inferior surface of the urethra. By the same opening the menstrual fluid escaped, as Beclard ascertained on one occasion by personal examination. She had menstruated regularly from the age of eight years; considered herself a female, and preferred the society of males."

"In this interesting case," says Professor Simpson, "we have present all the secondary sexual characters of the male, with some of the female genital organs developed in so excessive a degree as to approach, in several points, the more perfect structure of them in man. The impossibility, however, as mentioned by Beclard, of finding any bodies like testicles in the labia, or in the course of the inguinal canals, and more particularly the well-ascertained fact of the individual menstruating, can leave no doubt as to the nature of her sex."

In coming to a correct decision in doubtful cases we shall derive much assistance from an inquiry into the history, the passions, habits, and tastes of the individual, which, however, may be somewhat modified by the education he has received. It will not be necessary for my purpose to describe the other varieties of spurious hermaphroditism, such as extroversion of the urinary bladder, and inversion of the womb, &c., which will be found fully explained in the admirable article on this subject in the Cyclopædia of Anatomy and Physiology, by Professor Simpson, from which I have already quoted. I shall now, before proceeding further, detail the particulars of the case which has induced me to bring the subject before the profession.

Amongst the male convicts detained in the Richmond Government Prison there was one named Andrew R., aged twenty-six years, convicted of sheep-stealing at the Ballyconnell Sessions, on the 11th of April, 1848, and sentenced to seven years' transportation. Sometime after his conviction he was removed

to the Mountjoy Prison, where he remained from the 16th of December, 1850, to the 14th of April, 1851, when he was again sent to the Richmond Prison, his health having broken down under the severer discipline of the former. He now, for the first time, came under my care, and was admitted to hospital on the 15th of April, 1851, with symptoms of phthisis, which disease proved fatal to him on the 2nd of the October following. Although it had been for some time known to those about him that he possessed certain peculiarities of the organs of generation, I was only made acquainted with the circumstance two days before his death, when unfortunately it was too late, from his sinking state, to procure from himself such an account of his tastes, disposition, and habits, as would have added much interest to the facts I am enabled to state, and which I had much difficulty in obtaining, owing to the great unwillingness of the friends to give information about him.

It appeared that after birth there was considerable doubt in determining the sex to which the infant belonged, but it was at length declared, by a wise woman in the neighbourhood, to be a female, and accordingly was baptized by the name of "Anne." In a year subsequently, however, the organ representing the penis had so increased in size that a different conclusion was arrived at, and the name changed to "Andrew." After this period he was always looked upon and treated as a male, and as he grew up he excelled in several of the manly exercises. It does not appear, however, that he ever exhibited strong sexual desires, but as far as his predilections went, they were, according to his own account, for females, in whose company, he stated, a certain degree of excitement occasionally took place, during which the penis would become erect. How far this statement can be relied upon it is difficult to say, as he had a strong objection to have his imperfections known or alluded to in the slightest manner, even by his own immediate relatives. His voice was deep-toned and decidedly that of the male. His mother, however, is of opinion that he had

not much sexual inclinations of any kind, but that he was a male, principally from the fact of his having never menstruated. The appearance of this individual during life did not strike me as possessing the female character, though I had for several months before his death daily opportunities of observing him. It is true he was then broken down and emaciated, and the features altered from long-continued mental and bodily suffering. Shortly before his death, however, when aware of the hermaphroditic appearances, I thought I could detect a feminine expression in the features of the upper part of the face. The depth of the lower jaw, and the appearance of the teeth, partook more of the male, as did also that of the hair, larynx, arms, hands, thorax, lower limbs, and feet. There were but slight traces of beard, or of hair over the body, the pubis was covered with scarcely the usual quantity. The hip-bones were prominent, and the pelvis fan-shaped and otherwise decidedly female, but all the plumpness natural to that sex, if it ever existed, had now totally disappeared, as had also the mammæ, of which glands the rudiments alone remained.

The external generative organs presented the following peculiarities:—The penis, or organ representing it, was not under the usual size of that organ in the adult, and was supplied with glans, corona glandis, and prepuce, of the normal formation in the male. It was, however, imperforate: a rudimentary opening alone existing in the site of the orifice of the urethra, admitting a bristle to be passed for about a line. I may here mention that on dissection after death the body of the penis was found to be composed of corpora cavernosa, separated by the septum pectiniforme, and attached above by the triangular ligament to the pubis. The cruræ were well seen, converging from the rami of the pubis and ischium to the body of the penis which derived its supply of blood from the usual sources. The further dissection of the organ brought into view a substance much resembling the corpus spongiosum urethræ, which could be traced forwards towards the glans, and backwards to the pe-

rineum, where it became bifurcated, enclosing the orifice of the urethra, behind which its divisions again became united, and here terminated in a vascular spot not far removed from the situation of the bulb in the male. On further dissection, the transversus perinei muscle and artery were observed, but no trace of the prostate or of Cowper's glands could be found.

The female organs were represented, in the first place, by rather well-developed labia, each terminating somewhat abruptly behind, the fourchette not being present. Within the labia were seen the nymphæ, having much the same relations to the under surface of the penis as they have to the clitoris in the female. Next was seen the orifice of the urethra, before alluded to, as enclosed between the bifurcations of the substance resembling the corpus spongiosum, presenting the appearance of a longitudinal slit, and leading directly, as in the female, to the urinary bladder. Behind this orifice was placed another opening of a more circular form, and leading to a canal in the direction of the female uterus, and separated in front from the urethra, and behind from the rectum by distinct septa. A very perfect crescentic fold occupied the site of the hymen, and encroached so considerably on this vaginal opening as to prevent the introduction of a larger instrument than a No. 8 catheter. The female character, generally, may have been somewhat modified by the extreme emaciation present; and from the same cause, perhaps, it was, that I could find no trace of the mons veneris[a].

[a] The accompanying plates give an exact representation of the parts as they existed externally, and as they were shown by dissection and microscopical examination.

Plate I. Fig. 1.—*a*. The penis, supplied with glans and prepuce. *b*. The labia externa terminating abruptly behind. *c*. The nymphæ. *d*. The meatus urinarius. *e*. The vaginal opening, partly closed behind by the hymen. *f*. The anus.

Fig. 2.—Represents the section of the ovary as it appeared under the microscope. The granules visible are not the "Graafian vesicles," but appeared to be fat globules.

Plate II. Fig. 1.—Represents the parts as they appeared on dissection from behind, the sacrum having been previously removed. *a*. The rectum thrown back. *b*. The uterus. *c*. The left Fallopian tube passing backwards and inwards from the

PLATE I.

Fig. 1.

Fig. 2.

DUBLIN HODGES & SMITH, 1852.

On opening the cavity of the abdomen, my attention was first directed to an examination of a glandular body which had been observed and felt externally to occupy the internal abdominal ring on the right side, and which, consequently, from its shape and size, as well as situation, might be supposed to be a non-descended testis. It, however, proved to be nothing more than an enlarged lymphatic gland, lying immediately behind but not connected with the spermatic cord as it passed out of the ring. The cord on this side was much larger than on the left, where it resembled the round ligament in the female. On the right side it could be distinctly felt externally, descending from the ring towards the pubis. On dissection in this part of its course, it was found not to contain either the vas deferens or spermatic vessels; and at its termination it was connected with a small sac, containing fluid, near the pubis. From the internal abdominal ring the right spermatic cord proceeded backwards, and inwards towards the fossa or *cul de sac*, between the bladder and rectum, in which region a very remarkable condition of parts presented itself. On drawing forwards the bladder, it was evident that some firm, resisting body intervened between it and the rectum, which, on closer observation, proved to be a well-formed but small uterus, bearing the exact relations to the bladder, rectum, and peritoneum which it does in the normally-formed female. It was now found that a No. 8 catheter could be passed from the vaginal opening before de-

left cornu, between the rectum and uterus. *d.* The corpus fimbriatum. *e.* The ovary. *f.* The broad ligament of the left side and the round ligament. *g.* The bladder. *h.* The testicle, pendulous into the true pelvis, with a portion of the tunica albuginea dissected off. *i.* The plexus pampiniforme connected with it. *k.* The epididymis. *l.* The vas deferens turning back to reach the uterus. *m.* The point at which it enters that organ. *n.* The right spermatic cord proceeding to the ring. *o.* Glandular body lying behind it, and partly in the ring.

Fig. 2.—The tubular structure of the testis, as it appeared under the microscope.

Fig. 3.—Seminal fluid as it appeared under the microscope, exhibiting the cells and granules described by Wagner, as proper to that fluid, but showing no trace of "spermatozoa."

scribed into the cavity of the uterus. The broad ligaments were seen connected with the round ligament on the left side, and, to a lesser extent, with the spermatic cord on the right,—my reason for so designating the latter will become apparent when I come to explain its connexion with the male reproductive organ not yet described. The Fallopian tube, which was single, passed from the left cornu of the uterus backwards and inwards between the rectum and the body of the uterus, to the right of which it terminated in a large and well-formed corpus fimbriatum. In this course the Fallopian tube was tortuous and twisted on itself, and contained between its convolutions an oblong substance, which, on examination, had the character of muscular fibre. During its whole course this tube was completely permeable, and opened freely at the corpus fimbriatum and into the cavity of the uterus. On lifting up the corpus fimbriatum it was found to rest on an ovary, lying near the right side of the body of the uterus, and firmly bound down in this situation by the peritoneum. No trace could be seen of a second Fallopian tube or ovary, on a very careful dissection.

About an inch and a half external to the ovary, however, I found a body, pendulous into the true pelvis, in front of the right sacro-iliac synchondrosis, and immediately behind the internal iliac artery, as it descends into the pelvis, which, after minute examination, I ascertained to be a testis. Applied to its anterior and inferior surface was seen the epididymis, rather longer and in a more unravelled state than usual, but still divisible into a globus major, a body, and a globus minor. The vas deferens was plainly seen emerging from it, taking a very remarkable course. It first passed forwards and outwards in the direction of the internal abdominal ring, towards which it reached about half way, then turned back, forming a loop, the convexity of which looked towards the ring. It next took a course inwards, and somewhat backwards, in the direction of the uterus, to which it was finally connected by the broad ligament of the right side.

PLATE 2.

Fig. 1

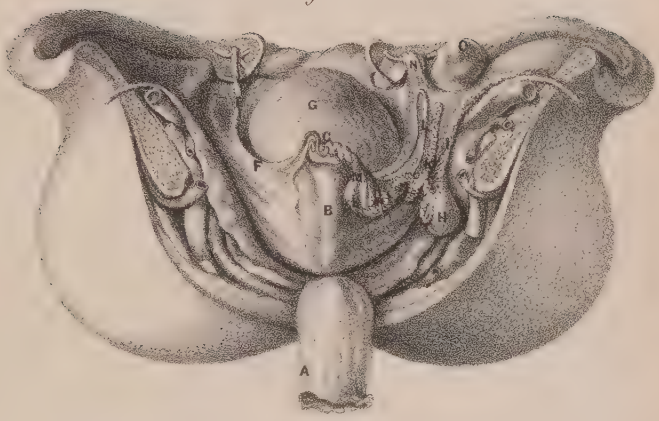


Fig. 2.

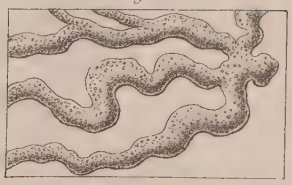
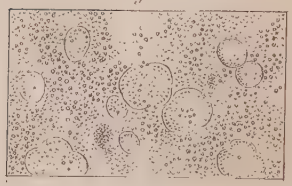


Fig. 3.



DUBLIN, HODGES & SMITH, 1852.

In the first part of its course the vas deferens was of good size, but as it approached the uterus became exceedingly fine and delicate. It could, however, be traced into the substance of this organ, which it perforated a short distance below its right cornu in an oblique direction towards the cervix, which was proved by mercury having been pressed through this tube into its cavity. The remarkable course taken by the vas deferens in this instance, and its final destination, are well worthy of remark, and will be again alluded to when considering the possibility of the occurrence of self-impregnation under this peculiar arrangement of organs. It is evident that there was a strong tendency in this duct, in the first instance, to pursue its usual course to the abdominal ring, towards which it may be presumed it was drawn by the contractions of the gubernaculum testis. A case is mentioned in which the vas deferens was drawn down into the scrotum by the same means, the testis still remaining in the abdomen.

Connected with the vas deferens I looked anxiously for the vesiculæ seminales, but in no situation could I find the least appearance of either of them. Neither could a second testis be found. The several organs and their relations are faithfully delineated, as they appeared on dissection, in Plate II. fig. 1.

The history of this case suggests two important points for consideration,—1st, As to the value of an examination during life, of the sexual malformations and secondary characters as they occurred in this instance, in enabling us to form a correct opinion of the predominant sex; and secondly, as to the probable physiological causes of the sexual duplicity and extraordinary arrangement observed in the dissection of the internal reproductive organs, and their possible effects or tendencies. Had I been called upon to pronounce an opinion as to the sex of Andrew R. during his life, I should no doubt have been impressed with the difficulty of deciding whether he was entitled to be considered an hypospadic male, or a female with an enlarged clitoris; and in such cases the importance attached to

our decision cannot be overrated, for on it will depend the destiny and happiness of the individual concerned, as well as those with whom he may become connected. In a medico-legal sense also its importance is obvious, as the unfortunate individual here concerned might have found had he raised the point of doubtful sex previously to his sentence.

It is, at all events, certain that this individual never had, nor scarcely could have, *effectual* connexion with either male or female. With the latter, the imperforate state of the penis would render it impossible, which is by no means the case in all cases of hypospadias, in some forms of which the under surface of the penis is grooved, and thus capable of conveying the semen to its destination, as must have been the state of parts in the case of the father and son both affected with hypospadias, before-mentioned. Connexion in this case with a male was rendered almost impossible by the narrowness of the vagina, which, as before stated, would only admit a No. 8 catheter; but further than this there was no reason why it might not occur. The primary fault in development giving rise to hypospadias has already been explained; and it may here be mentioned that an incomplete union of the symmetrical halves of the embryo at the perineum is natural to the female, in whom the vagina remains as a remnant of the original division. In this case the narrowness of the vagina and the encroachment of the hymen denoted a greater advancement towards complete union than is observed in the normally-formed female. The secondary characters of the male prevailing in the external conformation of the body, together with the appearance of a gland like the testicle at the abdominal ring, and the absence of menstruation, might have led to the belief of Andrew R. being really a man, with hypospadic malformation. We have, however, seen the secondary characters of the male also prevailing in the case given by Beclard of Marie Lefort, who was, notwithstanding, a female, as proved by her regular menstruation. As regards tumours, similar to the testes, having been occa-

sionally found in the cleft scrotum or inguinal canal, it is no proof of their being in reality such. They may be, as it turned out in this case, merely enlarged lymphatic glands; or one or both ovaries may descend into the labium on each side, as occurred in a case the preparation showing which is at present in the Museum of the Royal College of Surgeons of Ireland, and be taken for testes. In some instances they have been excised by operation from this situation to remove the inconvenience created by their presence. The presence or absence of menstruation will be a most valuable guide, but still an uncertain one, as we know in some females it never appears. Its absence in the present instance can be accounted for, supposing the individual a female, if we coincide in the opinion of Dr. Lee, that this evacuation depends on periodical changes in the ovaries, connected with the Graafian vesicles; for, on dissection of the single ovary here present, and examination under the microscope, no traces of these vesicles could be found. The structure of this body when cut into, showing the stromal tissue, as well as its shape and connexion with the corpus fimbriatum, proved that it was an imperfect ovary. It, as well as the other organs described, was examined by several professional friends, none of whom entertained any doubt of their being such as I have explained them. They are well preserved in a preparation now at the College of Surgeons, from which an accurate drawing was taken by Mr. Conolly, as represented in the accompanying lithographs. The uterus here could not be mistaken for an enlarged or unfolded prostate gland, or united vesiculæ seminales, which malformations have given rise to doubt in other instances.

Long ago it was supposed that the generative organs of one sex were typical of those of the other, and that the female was nothing more than an inverted male; but, without going to that extent, there is reason for supposing that a strong analogy exists between them. Thus the ovary represents the male testis, and the uterus the prostate gland, or, according to others, the

vesiculæ seminales. It is quite true that the male and female reproductive organs—the testes and ovaries—are originally formed in a manner so closely analogous, that in their earlier development they cannot be distinguished from each other, and in after life preserve a strong resemblance in their general appearance, and also in the remarkable part they are destined to perform in the reproduction of the species. The matrices by which they, as well as the kidneys, are originally formed, are called the "Wolfian bodies," which are large, and lie one on either side of the spine, and the defective arrangement of the reproductive organs in this case must be referred to an error of function in these bodies. Of the causes which may influence or produce this want of unity of action in them, we are as much in the dark as we are of those of the various other anomalies or monstrosities we so often meet with elsewhere. Dr. Knox supposes that the type of the sexes in the embryo originally possessed the character or elements of both sexes, the development of one or the other being determined by some influences or circumstances occurring afterwards, the nature of which he is unable to explain. This theory will not account for the cause of the duplicity or plurality of one sex or the other, which we know does occasionally take place. On the contrary, if we admit the theory, it is manifest that such a state of things could not take place without a departure from it.

The testis in this instance presented in a remarkably perfect manner the anatomical characters of that gland in the well-formed male. The tunica albuginea was first carefully dissected off, and then came into view the delicate tunica vasculosa and the tubular arrangement proper to the testis, which will be at once recognised on reference to Plate II. fig. 2, showing this structure as it appeared under the microscope. As further proofs of its identity, I may mention its connexion with the epididymis, which was itself easily recognisable from its partially unravelled state, and also the source from which it derived its supply of blood. The spermatic artery was traced

from its origin to this gland; and the plexus pampiniforme, formed by the commencement of the spermatic vein, can be seen in Plate II. fig. 1, injected with mercury, closely connected with it; the left spermatic artery passed towards the left cornu of the uterus, where it divided into three principal branches, one supplying the left side of the uterus, and communicating with the vaginal arteries; the second, conveyed by the Fallopian tube to the corpus fimbriatum and ovary, which it supplied; the third passed in the direction of the round ligament, towards the left abdominal ring. The left spermatic veins were small, and there was no appearance of the plexus pampiniforme. It has been suggested that the corpus Wolfiani occasionally does not become absorbed completely, and that the remains of it might possibly be mistaken for a testis, particularly as it then consists of a collection of convoluted diverticula, not unlike the tubular structure of the testis; but here the characters are too well marked to admit of such a doubt. Neither could the vas deferens be mistaken for the remains of its duct, known as the "duct of Gaertner," an error into which it is said Hunter fell in his description of what he considered the vesicula and vas deferens of one of his free martins.

On a review of all the circumstances connected with this case, and a mature consideration of the nature of the organs present, I believe I am justified in setting it down as an instance of that form of true blending together of the sexual organs described by Dr. Simpson under the head of "Lateral Hermaphroditism," with a displacement of the left ovary to the right side. Dr. Simpson's other divisions of true hermaphroditism are into the "transverse," and "double or vertical," but to them it is not necessary that I should further allude.

The case given by him, approaching the nearest to the one I have detailed, is that related by Sue[a]:—

"The subject was a young person of thirteen or fourteen

[a] De Hermaphroditis, Paris, 1749; Cyclopædia of Anatomy and Physiology, vol. ii. p. 698.

years of age. Of the internal genital organs there existed on the left side a very distinct ovary, a round ligament which ran to the groin of the same side, and a well-formed Fallopian tube, with its usual fimbriated extremity. The other extremity of this tube terminated in the fundus of the uterus, which occupied its usual situation between the rectum and bladder. On the right side again there was a slender elongated testicle, which had moved forwards to the corresponding inguinal canal, but had not proceeded so far as to pass out of the abdominal cavity. On the superior part of the testicle was a body resembling the epididymis, and the testicle itself sent off two tubes, which afterwards united into one immediately before its insertion into the uterus. The external genital organs were those of a hypospadic male, and during life the person had been always looked upon as belonging to the male sex. The perineal canal, or vagina, terminated between the scrotum and the root of the penis in a very small opening, which was common to it and the meatus urinarius."

This case bears a strong resemblance to that which I have related, both being instances of "lateral hermaphroditism," or a development of one or more of the reproductive organs of both sexes on each side of the body; a malformation of rare occurrence in man, as indeed are also the other varieties of true hermaphroditism. As we descend in the scale of animals, however, we find a departure from the distinct type between the sexes usually observed in man becomes less rare, until in some of the lowest classes double sex, or an hermaphroditic state, cannot be considered a monstrosity, but appears to be the natural condition, as is also observed in certain plants, particularly in the phanerogamia. It is a curious fact, that the earlier we examine the human embryo, the less distinct will be found the line of demarcation between the sexes, until in its more primitive stages they are, to our senses at least, anatomically neuter or hermaphrodite; thus, in this respect, bearing a somewhat similar relation to the perfect human being,

as do certain of the lower classes of animals and a portion of the vegetable kingdom. Dr. Simpson remarks that, "in the animal kingdom we find instances of a perfect hermaphroditic structure, as the normal form of the sexual type in the trematodes and cestoides among the entozoa, in the abranchial annelida, in the phanaria, and in many of the mollusca, particularly in the pteropoda, and in several families among the gasteropoda. In some of these animals that are thus naturally hermaphroditic, the fecundation of the female organs of the bisexual individual is accomplished by its own male organs; but in others, although the anatomical structure is strictly hermaphroditic, yet the union of two or, as it sometimes happens, of more individuals is necessary to complete the sexual act; and during it the female organs of each are respectively impregnated by the male organs of the other."

In the lepidoptera and other tribes of insects, and in the crustacea, lateral hermaphroditism is of very frequent occurrence. Amongst the latter Dr. Nicholl mentions a remarkable instance in the lobster, which possessed perfect male and female organs, placed respectively on each side of the body, the right side of which externally presented the characters of the male, and the left that of the female. And here, let me observe, that in those cases of lateral hermaphroditism observed in man, as well as in the various insect tribes, there is a decided tendency to a development of the male organ on the right, and the female on the left side. This obtains in by far the greater number of those instances which have been examined, and is a circumstance difficult to be accounted for. It will be recollected that in the case I have detailed, not alone was the male reproductive organ placed on the right side, but the female organ, connected as it was with the left cornu of the uterus by the left Fallopian tube, was also placed to the right of the uterus, from which I would infer not only an error of function of the corpora Wolfiani, but also a remarkable displacement of the organs resulting from them. An examination

of the skull in this instance throws some light on the physiology of this curious anormality.

In its general conformation the skull has many of the characters observed in the female subject. There is, moreover, a fineness of outline, the ridges not being so well marked as in the male cranium; but the point to which I wish most particularly to refer is the unequal development of the occipital regions on each side. On the left side the skull is here perceptibly more prominent than on the right, and from its greater capacity must have contained a larger mass of the cerebellum. Granting the supposition that the development of the generative organs is regulated by the influence of this portion of the brain, and admitting the fact of the decussation of the cerebral fibres, we have here a direct physiological reason why the reproductive organs should have been developed with a tendency to the right side of the body. In the celebrated case given by Mayer, of Marie Derrier, or Charles Doerge, in whom the internal as well as the external organs were so mixed that it was impossible to come to a satisfactory conclusion as to the predominant sex, Professor Mayer remarks[a], "that on the right side of the body there was a well-formed testicle, and on the left an imperfect ovary; the right sides of the cerebrum and cerebellum, but particularly the latter, were found to be smaller and less developed than the left, and the left side of the occiput was externally more prominent than the right." In this respect the case of Doerge bore a strong similarity to that of Andrew R., in whom, however, there was a still greater tendency to development of the reproductive organs on the right side of the body, the cerebral mass being greater in both instances on the left side.

The next point of interest suggested by the case of Andrew R. is, how far the state of the several organs of generation, and the relation they bear each other, approached that condi-

[a] Gazette Médicale de Paris (1836), No. 39; Cyclopædia of Anatomy and Physiology, vol. ii. p. 699.

tion of parts in the class of true hermaphrodites which contain within themselves both male and female organs; the former being capable of acting on the latter, and producing self-impregnation, as occurs in certain of the lower tribes, as amongst the mollusca, pteropoda, annelida, and common caterpillar. In Dr. Simpson's essay there are mentioned two cases, as occurring amongst the mammalia, "one of a sow, with perfect male organs on one side, and a womb containing several fœtuses on the other (Linnæus); and the other, of a rat, in which there was a co-existence of ovaries, and a uterus with nine fœtuses, along with complete male organs (Faber)."

If the reader will bear in mind the arrangement of parts as they existed in Andrew R.'s case, he will suppose with me that the testis or male organ here should become so excited by attempts of the individual at connexion with a female, or at masturbation, as to cause its secretion to pass from it through the vas deferens into the uterus, and thence be conducted by the Fallopian tube to the ovary. Here it must be acknowledged that we have present in a great degree the anatomical conditions necessary for self-impregnation, but before arriving at the conclusion of its possibility, other considerations must be entered into, viz., as to the perfection of the semen in its germinating elements, and of the ovary in a similar point of view. With some difficulty a small quantity of the fluid contained in the commencement of the vas deferens and epididymis was obtained and placed under the field of the microscope. It had the peculiar odour and consistence of the human semen, but was not in sufficiently large quantity to submit it to any chemical test. On the most careful microscopic examination the appearances delineated in Plate II. fig. 3 were found, on reference to which it will be seen there are numerous cells containing granules floating about in the liquor seminis, but at no point can be observed any traces of the spermatozoa. The characters either in form or in the motion peculiar to these remarkable bodies were completely absent, which, however, should cause no surprise if we

consider the history and sexual malformations of this individual, and the circumstances under which these spermatozoa are developed and called into action. It is well known that they are only seen when man has arrived at generative maturity, and in many animals during the rutting or procreative periods only. They are also said to be absent in certain hybrids in which the generative organs are in an imperfect state. We could, therefore, scarcely expect to find them in one possessing such imperfect organs as Andrew R. The granula seminis described by Wagner were, however, present, as I before mentioned, and we may here look upon them and their enveloping cells as similar to those found in the semen of animals at periods when their procreative powers are dormant, these latter being the primary or formative cells, producing the spermatozoa when their action is required on the arrival of the procreative periods. The imperforate penis of course rendering Andrew R. incapable of effective coition with a female, debarred him from the indulgence of the passions of the male (if they existed), and this affords another reason why the spermatozoa should not here exist, so immediately concerned are they in the generative function. Again, when we come to examine the ovary or female reproductive organ, we can find no trace of ova or Graafian vesicles,—as is evident on referring to the microscopic plate showing the structure of this organ, Plate I. fig. 2,—which in the female play so important a part in generation, and bear so striking an analogy to the male procreative elements. It is plain, therefore, that notwithstanding the presence in this instance of so many of the necessary anatomical conditions, self-impregnation was not for the above reasons possible.

In speaking of a somewhat similar condition of the sexual organs in a young ape which he had dissected, Dr. Harlan, of Philadelphia, remarks: "Admitting, what in reality appeared to be the fact, that all the essential organs of both sexes were present in this individual, had the subject lived to adult age most interesting results might have been elicited. Could not

the animal have been impregnated by a male by rupturing the membrane closing the vulva; or by masturbation, might not the animal have impregnated itself, by this means exciting the testicle to discharge its seminal liquor into its own vagina"? Supposing that the procreative elements would have been in this instance also imperfect, I should certainly answer Dr. Harlan's question in the negative for the reasons I have given above[a].

Art. VII. — *Clinical Reports and Observations on Medical Cases.* By J. T. Banks, M. D., M. R. I. A., King's Professor of the Practice of Physic, Physician to the Whitworth and Hardwicke Hospitals[b].

FARCY FOLLOWED BY ACUTE GLANDERS.

A man, aged 36, a groom, was admitted into the Whitworth Hospital, on the 1st of July, 1851. Three weeks before he sought for admission, he was affected with pains like those of rheumatism, but soon recovered, and remained well until the 16th of June, when he suffered from general "*malaise*," a sensation of "soreness" of the bones, disinclination to work, was chilly, and occasionally shivering in the day, and at night bathed in profuse perspiration. On the 20th of June he experienced a stiffness in his limbs, which made progression extremely difficult; he now observed that his arm and leg were the seat of painful swellings.

On admission he laboured under slight febrile symptoms; a hard, circumscribed swelling existed in the calf of each leg, with deep-seated, evident fluctuation; skin of a pink hue, and œdematous. On the outer side of the right thigh there was

[a] The drawings, casts, and preparations of the case of Andrew R. are at present in the Museum of the Royal College of Surgeons of Ireland. I should be ungrateful were I not to mention the great obligations which I am under to my friend Mr. W. Carte, for the valuable assistance which he rendered me in their preparation.

[b] Continued from vol. xii. p. 64.

an induration of the same character, ill defined in its outline, tender to the touch, and appearing to be situated under the fascia. On the left arm there were three tumefactions resembling those upon the legs.

Mr. Hamilton opened the abscesses in the calves of the legs and gave exit to a considerable quantity of purulent matter deeply tinged with blood. In different situations in the legs and arms depôts of purulent matter appeared, they were slow in their progress, and not attended with much inflammatory action: it was remarked that the abscesses almost invariably presented themselves at corresponding sites, so that whenever one appeared at any particular point on the leg or arm it was followed immediately by another in all respects symmetrical. The expression of the countenance was anxious; the surface of the body presented a slightly icterical appearance; evening rigors and night sweats were constant.

The progress of the case was very slow: no less than thirty-six abscesses were opened during his residence in the hospital. The character of the evacuated fluid varied very much. In some the contents were like those of ordinary abscess, in others there was a fluid of a sero-sanguineous character, and in one the matter closely resembled the secretion of the synovial membrane; ill-conditioned ulceration followed the opening of the abscesses of the legs; it was long before the ulcers showed any disposition to heal, but finally they assumed a healthy aspect, and at length complete cicatrization took place. There had been much destruction of the areolar tissue; and the motion of the muscles was considerably impaired; however, after some time, although the legs remained stiff, he was able to walk with the assistance of a cane. His health and strength were slowly reinstated.

In the treatment of the case the means best calculated to support the flagging powers of life were adopted. The diet was of the most nutritious kind; quina was administered; and cod-liver oil, in the latter and ulcerative stages of the disease,

was freely given, and was followed by a decided improvement in the state of the patient. He left hospital on the 12th December, 1851.

On the 29th of April, 1852, he applied to be taken into the hospital again, and upon examination he appeared to present the ordinary symptoms of continued fever. He was admitted into the Hardwicke Fever Hospital. He stated that he had been ill for ten days previously, and had a shivering fit every second or third day; his look was wild, and expressive of suffering; skin hot; pulse rapid; tongue white and loaded; thirst; his mind was clear and collected; the skin of the nose was of a pink colour, and there was exquisite tenderness on the slightest pressure, particularly on the right side. The voice was hoarse and husky, and on examination of the throat the palate was found to be extensively ulcerated, the surface being irregular, and of a deep livid hue.

May 3rd. Disease rapidly spreading; inflammation extending from the nose over the entire face; pustules thickly grouped together have appeared on the alæ nasi; the interior of the nares is in a state of phagedænic ulceration; and ulceration of the throat had made considerable progress; tongue covered with sordes; slightly jaundiced appearance of the skin; small pustules surrounded by a white areola were observed, chiefly on the back and shoulders, a very few were seen on the chest.

On the 4th and 5th there was an aggravation of all the symptoms, local and general. The ulceration of the throat and nose especially had made rapid strides.

6th. Much greater swelling of the face; the forehead of a livid colour, and œdematous, with pustules upon its surface; throat swollen externally; eyes nearly closed; deglutition very difficult; pulse 130, and feeble; tongue dry, and covered with a black crust; body emits a peculiar and remarkably offensive smell.

7th. Skin of a deeper yellow; occasional raving, but an-

swers coherently when roused; subsultus tendinum; extreme exhaustion: death.

Post Mortem.—Surface of the body of a deep yellow colour; the same disagreeable smell which existed before death still remains; the nose of a livid purple tint, and in a state of gangrene; anterior part of the scalp studded with dark livid spots and pustules; the white areola distinctly visible around each little pustule on the back and chest; the lungs did not present any lesion; the brain also was healthy; two small purulent depôts, one about the size of a walnut and the other smaller, were found in the substance of the spleen.

That glanders is capable of being communicated from the horse to the human being has long since been proved by the most irrefragable evidence. From the period (1810) of the first observation by Waldinger of the transmission of the poison of glanders to man down to the present time, a considerable number of cases has been recorded. That it existed in all times is more than probable, although we have no authentic ground for arriving at this conclusion. According to Ricord, Acton, and Beau, the disease which appeared at the siege of Naples in the fifteenth century was a *melange* of syphilis and glanders. The theory is supported by some plausible arguments founded upon historical documents. This, however, is a matter more of antiquarian curiosity than of practical importance. I am induced to publish the present case, not as a mere illustration of glanders in the human subject,—for of this disease we see many examples in the hospitals of the House of Industry,—but as a case marked by some extraordinary circumstances; and therefore worthy of being separated from the cases which are being occasionally placed under our observation. Glanders, as we generally meet the disease, is acute in its progress, running its fatal course in a very few days,—the subject of it being generally stricken down in the plenitude of health and strength. I do not know that I have ever seen a case exactly resembling that which is above briefly reported.

Having ascertained that the poison of glanders was absorbed by scratches received while administering balls to a horse which subsequently died of that affection, I could not for a moment doubt the accuracy of the conclusion I arrived at as to the nature of the chronic disease. In the first place, we have the fact of the reception of the poison; the manifestation of farcy; numerous tumours, painful and some fluctuating, forming in various parts of the body, a few disappearing without the actual formation of pus, the greater number being opened, and terminating in unhealthy and indolent ulceration; recovery, or, perhaps, more correctly speaking, apparent recovery; a calm; then an outbreak, as if during the period the poison had been dormant it had suddenly acquired increased intensity; the termination of the disease in acute glanders removing any uncertainty (if indeed such could exist) as to its primary source.

There are cases presenting all the phenomena of well-marked glanders, in which we fail, after the most searching investigation, to discover that the individuals so affected have ever come within the sphere of the poison. In illustration of this, I may refer to the case of a policeman, who died in the Hardwicke Fever Hospital some time since. Of the many examples of glanders which I have seen I never witnessed what appeared to me a more malignant and rapidly fatal form of the malady. Pustules were scattered over the body, and surrounded by the *white areola*, a remarkable phenomenon, the presence of which Dr. Hutton, I believe, first pointed out in the eruption of glanders. In this instance I noticed that the white areola became more distinctly visible after death than it had been before. The inability to trace the disease to communication with either horse or man labouring under glanders led me to practise inoculation with the matter taken from one of the pustules on a horse procured for the purpose. The result was the animal's becoming affected with acute glanders, and dying on the tenth day; the morbid appearances in each being

absolutely identical, purulent depôts existing in the lungs of both. If, in default of evidence, we admit that the disease in the policeman was not the consequence of the absorption of the poison of glanders, then we have unequivocal grounds for stating that a poison generated in the human body is adequate to the production of a disease in the horse *not distinguishable* from glanders.

My friend, Dr. Frazer, has published in the Dublin Medical Press a most interesting paper, in which his object is to prove the identity of glanders and diffuse inflammation: he reports cases of diffuse inflammation, which he observed while acting as my clinical clerk in the Whitworth and Hardwicke Hospitals, in which there existed the eruption with the white areola, considered to be pathognomonic of true glanders. Dr. Frazer, after stating the points of analogy between the cases of diffuse inflammation adduced by him and undoubted cases of glanders, asks: "Are they not identical diseases?" The question is still an open one. Would the matter taken from one of these cases have produced glanders? Perhaps it would. This is an inquiry of much interest, and one which I shall prosecute when an opportunity presents itself.

I will not here refer to treatment of glanders, believing that all the means which have been put in practice have alike ended in disappointing the hopes of the physician. The history of the disease presents all but an unbroken series of fatal cases. A solitary recovery from acute glanders is reported in the "Giornale delle Scienze Mediche di Torino," by M. Carnevale Arella. We are naturally curious as to the manner in which this exceptional case was treated. The treatment consisted of five venesections in three days, narcotics, and emollient cataplasms to the neck. The disease, the fatal progress of which we are in the habit of witnessing, is not one which would be likely to yield to such remedial measures.

To guard against a plague so loathsome and deadly, and so little amenable to treatment, how few are the precautions which

are taken! The popular notion is, that unless inoculation takes place the disease cannot be communicated from the horse to man. This is a mere delusion, unsupported by facts. Actual absorption, by means of an abraded surface, is not necessary; breathing the same atmosphere, having the fluids from the nostrils brought in contact with the face, as they sometimes are accidentally by the horse snorting; drinking water out of the same vessel,—by all or any of these means the disease may be propagated. The fact well known to veterinarians, namely, that the chronic and comparatively mild disease is capable of causing the acute, should lead to the enactment of more stringent measures, and to a stricter *surveillance* as regards horses suspected of being contaminated.

CHOREA TREATED BY CHLOROFORM.

For permission to publish the following case, I am indebted to Sir Henry Marsh.

The subject of it was a young gentleman, aged 12. He was a boy of more than ordinary intelligence, and had been in excellent health up to the time when he became affected with chorea. He had never laboured under infantile convulsions; neither at any period did he suffer from *rheumatism.* His father and mother are healthy, as are also his brothers and sisters, with the exception of one. About three months before the onset of his illness he received a wrench in the back while at play, but did not complain much of the effects of this accident. The first circumstance which attracted notice to his state was a manifest falling-off in his writing; his schoolfellows complained that they could not attend to their lessons when he was in their neighbourhood, for he never was quiet for a moment. It was soon observed that there was an unsteadiness in his gait, an awkwardness in his manner of moving his limbs. It was next remarked that his mouth was sometimes suddenly twisted; there were slight movements of the upper extremities, and more particularly of the fingers, which were in almost perpetual motion.

The will seemed not to have the slightest control over the muscular movements. Slowly the diseased action extended, until nearly all the muscles of the body were implicated; and the movements, which were at first comparatively tranquil, now assumed the character of extreme abruptness, and almost of violence. After some time there was utter inability on the part of the boy to carry food to his mouth; any *voluntary* effort was accomplished by a sudden jerk. There existed a state of the most perfect quiescence during sleep. The disease steadily and uninterruptedly advanced in intensity; progression became impossible; the muscles of the face were never for a second at rest; the mouth was twisted in all directions; the tongue was frequently bitten; the power of articulation was completely lost. The agitation was not confined to the face and extremities: the trunk participated in the movements.

The disease now attained its acme of intensity. To restrain the boy, it became necessary to have four persons, who were from time to time relieved by fresh relays. So violent were the spasmodic movements that even four strong assistants failed in their attempts to hold him down; and on one occasion he was dashed against his brother with such force as to prostrate him. The little patient now rarely slept, and never for more than half-an-hour. For four days and nights there was scarcely any intermission.

The skin of his back and wrists was abraded from the attempts to restrain his movements. It is unnecessary to detail *seriatim* the treatment which had been employed; suffice it to say, that remedial measures suggested by the experience of their success in numerous cases, had been fairly put to the test, and the disease nevertheless progressed, not being in the least degree influenced by the means adopted. Opium, so potent an agent in similar cases, received the most full and complete trial, carried to the utmost extent compatible with safety; it failed to induce sleep. Under these circumstances Sir Henry Marsh came to the conclusion that the case was one in which

chloroform might be administered; he requested my attendance, and in consultation with Mr. Cusack, it was determined to try the effects of chloroform as a "*dernier ressort.*" In the presence of Sir Henry Marsh and Mr. Cusack I proceeded to bring the patient under the influence of this anæsthetic agent. About half-a-drachm of chloroform was poured upon a pocket-handkerchief and held near the nose and mouth; after inhaling nearly two drachms in this manner, the muscular movements had altogether ceased, the right hand being the last part of the body which was reduced to a state of repose. The inhalation of the chloroform was not followed by sleep. After remaining motionless for some time the body became again agitated, but the movements were much less violent and abrupt. On the night after the exhibition of the chloroform the boy slept for at least five hours, not, however, without frequently awaking. The following day it was found that two persons were without difficulty able to restrain him. The chloroform was given as on the day before, and again he was reduced to a state of complete quietude. A good night's rest followed, but next day he was nearly as unquiet as ever. If he was released for a moment he seemed unhappy and evidently wished to be restrained. For two nights after there was scarcely any sleep. Chloroform inhalation was again applied and persisted in until profound, but calm and apparently natural, sleep was produced, neither lividity of the surface nor stertorous breathing being the consequence. The sleep continued for some hours, and on the following day there was a manifest improvement in all respects. The expression of the countenance was placid and composed; the movements of the limbs continued, but they were not abrupt and violent as they had been; food could now with facility be introduced into the mouth,—up to this time there was great difficulty experienced in feeding him; drink could only be given by means of a sponge, and at intervals small portions of jelly were thrust into his mouth; for the first time for many nights he slept without the aid of chloroform;

two grains of sulphate of quina were ordered to be taken every three hours; the legs were now rarely in motion; he could eat, and drink, and speak a little; hitherto, even after there was a decided improvement, he could only speak by monosyllables, and even these were violently jerked out. Such was the condition of the patient on the seventh day from the first exhibition of the chloroform. The improvement from this period was progressive; he gradually acquired the power of feeding himself, and he was soon able to walk about his bed-room. The only involuntary movements which continued were confined to the fingers.

He persisted in the use of the sulphate of quina, and after a short time he was sent to the country. The only trace of the disease which remained when he left town soon disappeared, and he now writes to his family that he is "quite well."

To satisfy ourselves of the frequently intractable nature of chorea, we have only to turn our attention for a moment to any of the standard works of reference on practical medicine, and there glance at the numerous and even opposite plans of treatment, which have from time to time been proposed for the cure of this disease, and highly eulogized by their respective advocates. That chorea presents itself under many and varied forms, and that one case frequently differs from another in its cause, its course, and its termination, is well known, and this sufficiently explains the fact, that opposite modes of treatment have been employed with like success. That many remedies have acquired a reputation in this disease which they do not merit, there cannot be a reasonable doubt. Experience abundantly proves that many diseases run a tolerably definite course and are little influenced by remedial measures. In this class it is justifiable to include many cases of chorea, at least of that which may be considered an acute form of the disease. The conclusion which M. Sée has come to is, that the mean duration of the disease is sixty-nine days, and that treatment has not much effect in shortening its duration. The opinion of

Sée is worthy of all respect, for he has, with great zeal and consummate ability, analysed no less than 128 cases of this disease. It being granted, then, that the tendency of chorea, in the vast majority of instances, is to a spontaneous restoration to health, we may easily perceive how very difficult a matter it is to test the real and undoubted value of therapeutic agents. The successful issue in some modes of treatment may be referred to the period of the disease at which they were commenced, and thus, on the "post hoc ergo propter hoc" principle, the physician is betrayed into the mistake of supposing that the last medicine employed has been pre-eminently powerful in the hitherto unyielding disease. That this often happens unwittingly, and without the slightest intention on the part of the reporter to mislead, must, I apprehend, be conceded. Nevertheless, among the many cases of chorea which come under our observation, we occasionally meet with some which neither time nor medicine seems to mitigate, and these cases, rare and exceptional doubtless, are however not quite so rare as one would be led to believe from a limited experience. Of the 128 cases of chorea recorded by Sée, 9 ended fatally. MM. Rilliet and Barthez have, in their work on the diseases of children, also described some fatal cases of this disease, and they have in an especial manner directed attention to the *progressive violence* of the muscular movements as indicative of danger. They remark: "Dans les cas ou la chorée doit avoir une issue *funeste*, on voit les mouvements acquérir progressivement une violence excessive, ou à peine alors à contenir les jeunes malades, mème en employant une force considérable. Ils brisent les liens dont ou les entoure, en un mot le désordre des mouvements est presque aussi grand que celui qu'on observe dans certains attaques d'épilepsie."

It is not my intention at present to enter into a disquisition on the treatment of chorea, but merely to remark upon the effects of chloroform in a case which resisted other means, and

which, I am convinced, would have eventuated in death if this powerful agent had not been brought into play. At the time that Sir Henry Marsh proposed the exhibition of chloroform in this case, we were not aware that it had ever been used in chorea, and he was induced to give it a trial solely on the grounds that ordinary expedients were found to be utterly powerless, and that a much longer absence of sleep and duration of the affection in all its fearful severity were incompatible with life. The results obtained more than realized any anticipation we could have formed of its probable effect upon the disease.

From a state of the most violent movement the patient was soon brought into a condition of perfect quiescence, followed by calm and refreshing sleep. Before concluding, it may not be void of practical utility to dwell for a moment upon the mode of administering the chloroform which was adopted in this case. In the first place, no apparatus was used; inasmuch as I conceive the best of all plans is the most simple, namely, that of pouring a little of the fluid upon a pocket-handkerchief, and bringing it near the nose and mouth. This is the method which I have seen followed by Professor Simpson, to whom the world is indebted for having first employed this anæsthetic agent, and thereby for having conferred an inestimable boon upon suffering humanity.

Free admixture of atmospheric air was permitted; at no time was profound insensibility induced. The symptoms to which the term "poisonous" is applied were not manifest even in the slightest degree. What influence chloroform would exercise over comparatively mild cases of chorea remains to be ascertained: the obvious conclusion must be, that it would be found an agent of singular efficacy.

On looking over the Number of the "Vierteljahrschrift für die Practische Heilkunde" of Prague for April, 1852, I find a case, taken from the "Deutsche Klinik," reported by

Grossheim. From the statement of the case, we may judge that it was one of ordinary severity. A child of nine years of age, labouring under chorea and affected with oft-recurring paroxysms, was treated by chloroform inhalations, after other treatment had failed, the paroxysms becoming more frequent and more severe. Chloroform (8–12 drops) was inhaled thrice daily. The paroxysms were diminished in number and severity, sleep was procured, and the patient was enabled to take food, and in eight days the muscular movements had almost ceased. In this case it was observed that an intermission of the chloroform was invariably followed by an exacerbation of the disease.

Much as the apprehension of fatal consequences resulting from the application of anæsthetic vapours has declined, still there are many medical men who entertain a deep-rooted objection to their use, and appear to be haunted by the spirits of those who have departed this life "poisoned" by some one or another of those, so to speak, noxious agents. The mere narration of such cases as the one I have brought forward may not be unfruitful of good results, if it were only to encourage those who are still sceptical as to the freedom from danger which attends the use of chloroform employed after the manner in which it was given in this instance. There are men so constitutionally cautious as to have a horror of all, or at least of many, remedies which are powerful for evil, not remembering, or not wishing to remember, that they are at the same time, if judiciously exhibited, powerful for good also.

In the case of tetanus treated by chloroform which I have lately published, and in this case, the chloroform was not given to the extent which is required in surgical operations, so that the danger which *possibly* may arise when profound insensibility is requisite did not in reality exist. In estimating the therapeutic power of chloroform in tetanus, chorea, and the cognate diseases, I have not lost sight of the small amount of proof we can adduce of its efficacy when numerically tested. True,

the cases are isolated; nevertheless, I conceive that we are warranted in the anticipation that future observations, and a more extended experience, will fully bear me out in the favourable opinion I have ventured to enunciate[a].

Art. VIII.—*On the Treatment of Typhus Fever by Sulphate of Quina*[b]. By John F. M'Evers, M. D., Extra Physician to the Cork Fever Hospital.

In presenting the details of a few interesting cases of typhus fever treated in the Cork Fever Hospital, it is due to Dr. Dundas of Liverpool, whose *brochure* on this subject led to their successful issue, to acknowledge the valuable service which he has rendered to the profession by the introduction of his novel and generally successful treatment of this formidable disease.

It would appear by a reference to Dr. Dundas' pamphlets, that he has had considerable experience of tropical fevers, and he states, what is confirmed by other writers, especially by military authorities, that quina will be almost invariably successful in the fevers of hot climates, if administered at an early period, and in sufficiently large doses[c],—twelve, fifteen, or twenty grains being a common dose in these latitudes,—repeated at short intervals, say every two hours, until dizziness of the head, or tinnitus aurium, be produced; an effect of the remedy, however, although characteristic and desirable, which

[a] Dr. Shekleton, Master of the Rotundo Lying-in Hospital, has been in the habit of administering chloroform in cases of puerperal convulsions since 1848. He tells me he has nine carefully noted cases of this disease in which he employed it. In five the convulsions were arrested, and in four the fits were lessened in intensity and frequency. Other modes of treatment necessary for the safety of the patients were, of course, combined.

[b] Read to the Cork Medical Society, March, 1852.

[c] See also on this subject Dr. Barton's essay on the Fever of Hong Kong, in the twelfth volume of the present series of this Journal, page 353.

does not always follow this peculiar mode of administering quina, and yet the disease may yield to the potency of a few doses.

By the success which has attended the use of quina in the hands of Dr. Dundas, he seeks to establish the identity of the typhus fevers of Europe with the remittents and intermittents of the tropics; and in reference to this part of his paper it is curious to find that, as far back as 1786, Dr. Skete, who wrote on the use of bark in fever, broached a similar opinion. He says: "If the remittents of warm climates are but the continued fevers of this country, in a more violent degree, and if the effects of the bark are admitted in such remittents, does it not necessarily follow that bark would be endowed with similar powers, even in the fevers which every day occur to our notice, I mean those of the typhus kind, which are frequent in all large towns, especially London and Edinburgh."

The treatment of typhus fever introduced into practice by Dr. Dundas, and to which I purpose to call attention in the following observations, consists in the administration of sulphate of quina in doses of ten grains, repeated every two hours, until dizziness of the head, or tinnitus aurium, shall be produced; or, should these evidences of the curative effects of the remedy not occur, the medicine is continued until a general amelioration of the state of the patient takes place,—broth and a small quantity of wine being allowed, purgatives, or even aperients, not being deemed necessary during the treatment. When the dizziness or the tinnitus are very urgent, Dr. Dundas resorts to an emetic, which, in my own practice, I have not made use of: he also states, that if emetics are had recourse to at an early period, the quina is likely to be more successful. This treatment is resorted to in all stages of the disease, and frequently in the advanced periods, under circumstances which would be considered by the experienced physician as indicative of the worst form of typhus fever; and

this mode of administering quina is almost invariably attended with the happiest results.

Immediately after having become acquainted with Dr. Dundas' views on this subject, several persons labouring under bad maculated typhus were admitted into our hospital, which gave me opportunities of testing the value of his opinions; and I must confess that I pursued the inquiry with much doubt, as I looked on some of the cures related by him to be of too marvellous a kind to justify my adoption of this treatment without further confirmation of its value: however, I have now tested the remedy in nine cases, and, with the exception of one, it has been signally successful. The first of these cases was the following, the notes of which I take from the daily reports of the hospital.

Pat Ryan, aged 28, a labourer, was admitted into hospital on 1st January, 1852, from Hop Island. His urgent symptom is headach; pulse 100; tongue foul; skin hot; had taken purgatives at home, and was treated since admission in the usual way with salines, ablutions, &c., until the thirteenth day of his illness, when the usual symptoms of bad typhus became apparent. On the previous day his skin was mottled, and now the entire surface has assumed a dusky hue. Pulse 112, feeble; tongue parched, with sordes on the teeth and lips; no sleep; bowels free; some general fulness of abdomen, with epigastric tenderness; kidneys acting; headach increased, and raves a good deal. He was ordered to take ten grains of sulphate of quina every second hour, and to have broth and four ounces of port wine.

The changed condition of this patient at my next visit was most remarkable: the pulse was considerably reduced; the tongue was moist and cleaning, in fact it had lost the dark colour and parched appearance it presented the day before. The man slept; there was less abdominal fulness and tenderness; the kidneys acted well, and the bowels were free. He took sixty

grains of quina without its having produced dizziness or tinnitus aurium. The medicine was given in the form of mixture with a little sulphuric acid,—the two or three last doses sickening him a little. I then ordered it in the same dose in the form of pills, repeating it every three hours, which he bore without sickness. On this day he took forty grains, and on the following day he was convalescent. It is remarkable that the father of this patient, who was admitted a few days before, passed through the same type of fever, treated in the ordinary way, and died on the fifteenth or sixteenth day of his illness.

The second case was that of a young man, aged 19, admitted on the 14th January, for some days under my care, whose urgent symptom was headach, with great prostration of strength. On the eighth day his breathing became very much hurried, unattended, however, with cough, nor did the stethoscope elicit any abnormal sound: the man being remarkably pallid, except during two short exacerbations which occurred in the twenty-four hours. I ordered him aromatic spirit of ammonia mixture, with a small quantity of wine, and a sinapism to his chest. The following morning, on examining the chest, I discovered a purple patch occupying the part to which the mustard had been applied. His respiration was improved, but he complained of intense headach. Pulse 108, and feeble; no sleep. The purple patch induced me to a careful examination of his body, and on turning him in the bed I observed the lower portion of the back and the nates covered with purple maculæ. The case I looked on then as well adapted for the administration of quina, and, accordingly, I ordered ten grains every two hours, together with broth, and two ounces of port wine. The third dose produced violent headach, with tinnitus aurium, when the medicine was discontinued. At my next visit, on the following morning, all his symptoms were improved; the tinnitus aurium left him in a short time after the medicine was laid aside, and the headach was greatly mitigated; he slept, and expressed himself much better. I placed him

again on the quina, when the third dose produced the same results as yesterday, but there was so great an amelioration of all his symptoms that I considered him proceeding to convalescence, and gave him only two grains of quina three times a day, after which he rapidly recovered.

The next two cases were females, both of whom presented unfavourable symptoms, and in whom the remedy was equally successful.

The fifth case was one of great interest, exhibiting other symptoms of an unfavourable nature, in addition to those enumerated in the preceding.

John Eames, aged 55, a smith, was admitted on the 23rd of January, with bad typhus, having been discharged from hospital on the 3rd inst., cured of ordinary fever with bronchial complication. On the ninth day of his second attack he became mottled and talked incoherently; on the tenth day he was not improved; he had no sleep; the tongue was parched and dark, and protruded with difficulty; stools involuntary; pulse 120, and feeble. He was ordered to take ten grains of sulphate of quina every second hour, and to have broth and two ounces of port wine.

Half-past 9, P. M. The quina was commenced at 1 o'clock; he has taken fifty grains, and appears improved in every respect; he is more collected and speaks with less difficulty; he has had two large voluntary evacuations from the bowels; the medicine was directed to be continued.

On the 11th day, I found that he had taken sixty grains since my last visit, and although he did not sleep much, and had some singultus in the night, he was much improved in other respects. The pulse had come down to 100; the tongue was still parched and brown, but he was perfectly conscious; he was very deaf and the maculæ were improved in colour. On this day he took only four doses of quina, it having been omitted on account of the "buzzing," as he expressed it, which the remedy produced.

12th day. Pulse 96; tongue moist and cleaning; bowels open, and he passes water freely; to take five grains of sulphate of quina every fourth hour.

13th day. Pulse 84; tongue clean; skin cleaning and scaly; convalescent.

I will not occupy time with the details of all the cases in which I have tried this remedy, but will content myself with the recital of one more case, which occurred within the last few days, and which I consider in every way worthy of observation.

Mary Delany, aged 22, admitted from Ballincollig on the 10th March; 9 days ill; headach; petechiæ; pulse 120, and very feeble; tongue parched; got the usual aperient of the hospital.

10th day. One stool; pulse 130, feeble; tongue parched and split; great thirst; respiration hurried; had no sleep; she is often flushed, and in the intervals deadly pale; countenance anxious, with the appearance of suffering; great fulness of abdomen, with tenderness on pressure, especially at the epigastrium; maculæ of a dark brown. She was ordered ten grains of sulphate of quina every second hour, and to have broth and two ounces of red wine.

11th day. Bowels not open; tongue moist and cleaning; pulse 100; kidneys acting.

12th day. At 8 P. M. last evening this girl became very stupid; "did not know what to do with her head," as she expressed herself; she also became deaf, and had tinnitus aurium; this state continued until midnight, with occasional sleep, when she became "lighter," and afterwards slept well; the kidneys have acted, but there is no discharge from the bowels; she now presents a totally different appearance from that of yesterday; she is free from headach, her respiration is natural, the tension and fulness of belly have disappeared, but there is still some slight epigastric tenderness; some of the maculæ

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have disappeared, and the remainder are of a lighter colour. The sulphate of quina to be continued every fourth hour.

13th day. No stool; tongue clean and moist; pulse 84; belly natural; slight epigastric tenderness. The quina to be omitted; broth and wine to be continued; to have a domestic enema.

14th day. Convalescent.

Since the first introduction into Europe of the Jesuits' bark in 1649, the most celebrated writers extolled its efficacy in the treatment of various diseases, but especially in fevers; owing, however, to some cause or other, it fell into disuse; and it is probable that this was occasioned by the many failures which attended its administration; failures which I now feel assured were owing principally to two causes:—first, the bark not having been given in sufficiently large doses; and secondly, the bark not having been always of the genuine kind. Its use was revived by the celebrated Sydenham, and after the time of that eminent man, its excellent qualities were fully established by Hoffman, De Haen, Pringle, Cleghorn, and other practitioners of eminence. And it is worthy of remark that those who were most enthusiastic in praise of this remedy gave it in very large doses. Dr. Clark, a celebrated physician who wrote on fever in the year 1770, was in the habit of prescribing bark in two-drachm doses every two hours, and at the same time of exhibiting it by the rectum in the form of enema; he relates that on one occasion in mixing a dose of bark for a patient, he discovered that the apothecary had by mistake put half-an-ounce of bark into each paper instead of two drachms, which he had ordered; the patient had been taking this large dose for a considerable time, and got cured of a bad typhus in consequence. Now, when it is estimated that two pounds of good bark will yield about two hundred grains of sulphate of quina, it must be admitted that Dr. Clarke's treatment of typhus was not far short of the heroic method of the

present day introduced by Dr. Dundas. It is unnecessary to advert to the fact that Dr. Clarke's patients were taking a different preparation, possessing, however, the same active base.

One great objection to its general use in this country, which may be advanced, is the high price of the salt; but should the remedy be found as efficacious in the practice of others as by Dr. Dundas and myself, I feel assured that its use, when applicable, would eventually constitute the true economy of our fever hospitals.

I have placed these cases and observations together in a very hurried and, therefore, imperfect manner, but the subject I considered to be of so much importance to the profession, as to induce me to take advantage of the earliest opportunity of bringing forward the results of my experience of this new treatment for typhus; and I do so in the hope, that this notice of the subject, however imperfect, may induce others to test the value of quina in this disease, in the large doses recommended.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Chapters on Mental Physiology. By Henry Holland, M.D., F.R.S., &c. Founded chiefly on Chapters contained in "Medical Notes and Reflections," by the same Author. London: Longmans. 1852. 8vo. pp. 301.

Few of our readers, we trust, are unacquainted with Dr. Holland's former work. Distinguished as it is for simplicity and purity of style, for an unusually severe logic, and for its great experience, it is not surprising that it should have been placed among the most classical works of medicine. Probably no one has risen from its perusal, without hoping that one so capable of affording instruction in so delightful a manner, would again be induced to give the profession the benefit of his large and intelligent observation of disease. And this desire has been fulfilled. Seven of the chapters of the former work have been rewritten, and four others have been added to make their "sequence and connexion" more complete. We are quite satisfied that the work will add to Dr. Holland's reputation as a close observer as well as an accurate thinker. Both qualifications are necessary to the character of a great physician: neither is common, and the two are rarely combined. The work is exactly what the title imports,—not metaphysics, though involving metaphysical inquiries, but including the complex conditions of body as well as mind, and their reciprocal action and intedependence, embracing much that is deeply interesting to the philosopher as well as the physiologist.

The first chapter is rather introductory, consisting of some very just remarks on the difficulties of medical evidence, and the profound knowledge and care requisite to assign to it its just value. Upon this we shall not dwell, but pass on to the se-

cond chapter, "On the Effect of Mental Attention upon Bodily Organs," which contains the key-note of the entire volume. What effect has the concentration of our attention upon one or more bodily organs? Superficial observers may be surprised at the question, but those who have seen much of a certain class of affections, and especially those who are in the habit of observing their own states of mind and body, know that very curious effects are produced. Thus, if by an act of the will, or by a semi-involuntary effort, the attention be fixed upon the organs of circulation and respiration, a disturbance will often result, from the mere direction of consciousness upon these organs, without any emotion or anxiety. So of other organs: the stomach, as most dyspeptics know; the alimentary canal; the bladder; the generative organs, &c. That certain disturbances are the result of directing our attention upon the organs of sense, and even upon the sensorium itself, we know from personal experience, though the analytic explanation of the latter is more complex, as Dr. Holland observes. We cannot but agree with him also in attributing certain phenomena which have been latterly exciting some attention—such as some of those of biology, and, perhaps, the mystery of the divining-rod, and certainly the experiment of the suspended ring mentioned by Mr. Mayo—to the effect of attention directed to the surface of the body or the muscular system.

"If now, dispensing with other examples, we turn for a moment to the elementary view of this principle with which we set out, our first impression will be that of its close connexion and co-ordination with every part of our existence, mental and bodily, intellectual and moral, under the conditions of health and disease. The agency in question is, in truth, so incorporated with us,—so integral a part of the mind in relation to the bodily organs as well as the whole class of mental emotions,—that it is difficult to make language subservient to its description, or to the points which need discrimination in its theory. The point especially meriting regard is the evidence afforded that the physical state or function of a part is actually altered from this cause, and not the mental perception only."

But what is the physical agent of this direction of attention? Dr. Holland thinks that it is probably through some of the nerves, but it is extremely difficult to say by what class. Not those of voluntary motion, for none is excited; and if by those of sensation, we must then admit—

"Two several actions in opposite direction along the same tract of nerve; a condition of which, though not disproved, we have no well-assured evidence. In neither of these functions, therefore, nor in the nerves ministering to them, can we find any certain ex-

planation of the phenomenon before us, though it has various and close relations to both, as they mutually have to each other."

Whether further investigation into the views of the reflex action of the brain, proposed some years ago by Dr. Laycock of York, may throw light upon this subject, it is hard to say. At present we quite agree with Dr. Holland, that we have no means of solving the difficulty.

The next question is one intimately connected with the preceding, viz. "On mental consciousness in relation to time and succession;" the difficulty of which is increased by the mind being at once the subject of our investigation, and the instrument by which such investigation must be made. The problem to be solved is:

"Whether our mental existence, as interpreted by consciousness, is best viewed and understood as a series of acts and states single at the same instant of time, succeeding each other with more or less rapidity of change, but in absolute and unbroken sequence?—or as a wide and mixed current in which various sensations, thoughts, emotions, and volitions do actually coalesce and co-exist as to time, and are simultaneously testified to us by this common consciousness."

At first, perhaps, many will incline to the latter opinion, from a consciousness of the multitudinous character of our mental operations, but we feel satisfied that those who are more practised in mental analysis, and more given to mental introspection, will perceive that the confusion arises from the variety, and the apparent simultaneity is resolvable into the infinite velocity, of thought and emotion. Let any one examine carefully the operations of his own mind in its more tranquil states, and we think he will scarcely doubt that our consciousness is of a series of sequences; and in like manner he may trace a distinct succession, though more dim from increased velocity, even when the mind is more excited, and its operations more tumultuous. If this is so, what power has the will in regulating the succession of these states? Comparatively little, according to Dr. Holland, although it varies in different individuals, and in the same person at different times. The whole question is ably discussed by the author, but we shall content ourselves with quoting the last paragraph.

"Can this voluntary power over the course and succession of mental states—thus varying in different individuals and limited in all—be exercised and cultivated in such a way as to enlarge its scope and give it greater energy to resist the causes which control it? Experience answers at once and unequivocally that it may be so. The faculty in question is given to us not merely to use,

but to educate and exalt. It is eminently capable of cultivation by steady attention of mind and habitual exercise; and rightly thus exercised, it becomes one of the highest perfections of our moral and intellectual being. By no quality is one man better distinguished from another than by the mastery acquired over the subject and course of his thoughts; by the power of discarding what is desultory, frivolous, or degrading; and of adhering singly and steadily to those objects which enlarge and invigorate the mind in their pursuit."

The next chapter, on "Time as an Element in Mental Functions," is devoted to showing forth the curious fact which each must have observed in himself at different times,—that the velocity of mental operations varies; and this is still more remarkably the case in certain diseased states, in which comparatively long intervals are required after an answer is asked before an answer is returned; not from difficulty in articulation, nor from want of intelligence, but from each step in the mental process being performed deliberately and with effort.

If familiarity did not destroy wonder, no doubt, as Dr. Holland observes, the ordinary phenomena of sleep would excite as much surprise as the induced sleep of mesmerism; for to those who observe accurately, there is much to interest both philosophers and medical men, and much which is difficult of explanation.

Our author considers sleep, "in the most general and correct sense of the term," not as—

"One single state, but a succession of states in constant variation; this variation consisting not only in the different degrees in which the same sense or faculty is submitted to it, but also in the different proportions in which these several powers are under its influence at the same time. We thus associate together under a common principle all the phenomena, however remote and anomalous they may seem,—from the bodily acts of the somnambulist, the vivid but inconsequent trains of thought excited by external impressions; the occasional acute exercise of the intellect; and the energy of emotion—to that profound sleep in which our impressions are received by the senses; no volition is exercised; and no consciousness or memory is left, on waking, of the thoughts or feelings which have existed in the mind."

This conclusion, at which our author arrives by a very careful train of reasoning, is further illustrated, and in its turn illustrates other analogous conditions, such as trances, catalepsy, mesmeric sleep, hypnotism, &c. The proximate cause of sleep is referrible to some condition of the nervous substance, or of the circulation in the brain and nervous system, and per-

haps of the quality of the circulating fluid. The careful investigation of the cause of sleep and of wakefulness, and its effects, opens a field of great importance to the physician, who will derive great help from the indications pointed out by Dr. Holland.

Perhaps the most interesting chapter in this very interesting volume, to a medical man at least, is the one on "Dreaming, Insanity, &c." The resemblances of the several states of dreaming, insanity, delirium, and intoxication are very remarkable.

> "A dream put into action (as in reality it is, under certain conditions of somnambulism) might become madness in one or other of its most frequent forms; and conversely, insanity may often with fitness be called a waking and active dream. Delirium and intoxication are transient effects, from temporary causes, of that condition of sensorium which, more deeply fixed and longer continued, obtains the name and produces all the aspects of mental derangement."

How, then, are we to define insanity? That this is a most difficult question is proved by the various attempts which have been made and their signal failures, by the confusion which prevails in legal investigations, and by the disagreement of the highest authorities. Dr. Holland's description, which embraces the greater number of cases of the disorder, is, that it consists in the

> "Loss, partial or complete, of power to distinguish between unreal images created within the sensorium and the actual perceptions drawn from the nerves, thereby giving to the former the semblance and influence of realities; and secondly, the alteration or suspension of that faculty of mind by which we arrange and separate the perceptions and thoughts successively coming before us."

This, however, does not include what has been called moral insanity, or madness whose seat is in the will. If we say that insanity is an error of consciousness,—first, as regards the intellect and senses; second, the affections; and third, an error of determination as regards the will, we shall probably include all the leading varieties of insanity. But even if we accept this as an abstract definition, it is to be feared that the difficulties in applying it to practice, especially to those cases where the insanity varies in degree, will be very considerable.

If we investigate the phenomena of dreaming, intoxication, trance, &c., we shall find startling similarities up to a certain point, and from this contemplation we may derive much light in the consideration of the fearful disorder in question. This

Dr. Holland has done at considerable length, and very happily we think, and we recommend such of our readers as take an interest in this subject to read carefully his remarks. But what is the physical cause of insanity? That it may, to a certain extent, depend upon the quantity of blood in the brain, its local distribution, the rate of circulation, or the quality of the blood itself, is true in some cases; but it is beyond doubt that there is some other element, some condition of the nervous substance itself, more than softening or induration, which has yet escaped all our means of investigation, upon which many, if not most, of the cases depend. Whether it is some change too intangible for chemical, anatomical, or microscopical investigation, or whether we have not as yet discovered the proper method of carrying on our investigations, we cannot say; but it is certain that we can give no explanation of the physical cause.

Dr. Holland observes, that—

"As the variety of the disorder presumes a corresponding difference in its cause, another suggestion may arise, whether certain kinds of insanity be not the effect of simple excess or deficiency in quantity of that power (nervous, sensorial, or however denominated) by which the mutual relations of mind and body are maintained? It is certain that such variation as to quantity exists; certain also that it produces much influence on both these parts of our existence. If we might suppose that some maniacal disorders depend on a morbid excess of this power, we should have an example of the case alluded to above, viz., derangement of mind proceeding from physical causes, but then involving only transient alteration in a natural function, and not any changes of structure which can leave permanent traces behind."

Connected with the cause of insanity, we cannot altogether leave out of sight the question as to whether it is simply and solely a disease of the body, or whether the mind itself may not become disordered as well as its bodily organ. Modern opinion leans to the former of these conclusions, but it is far from being proved; on the contrary, many of the phenomena we meet seem more explicable on the latter supposition. But this inquiry would lead us too far. To return to the exciting causes of madness,—the more common are familiar to all, and, therefore, need not be enumerated; but there is one upon which Dr. Holland dwells a little more, which is well worthy our notice:—

"It seems probable that certain cases of madness depend on a cause which can scarcely exist, even in a slight degree, without producing some mental disturbance, viz., the too frequent and ear-

nest direction of the mind inwards upon itself; the concentration of the consciousness too long continued upon its own functions. It would appear (if we may venture so to surmise), a design in the creation of this wonderful existence which we call soul, that, while safely using the faculties given to us in exploring every part of the world of nature without, we should be unable to sustain their powers when directed inwards to the source and centre of their own operations. I doubt not that the long continuance or frequent repetition of such circumstances, from whatever cause, is sufficient to produce a temporary derangement in minds already predisposed to the infirmity. I have myself known one instance of aberration of intellect which I had every reason to believe thus produced."

That faculty of the mind which appears least material is, perhaps, the memory; for if we remember the vast amount of impressions, apparently slight and unnoticed at the moment, but which are frequently recalled after a lapse of time, we can scarcely regard them as physical, and yet no faculty is more dependent upon the completeness of organized structure for its perfection. The will has only a partial control over it, but we are at once struck by the curious effects of disease upon it, without being able to explain them. Take the case of insanity:—how much is forgotten, and how much recalled that had previously been forgotten, and yet we cannot tell the reason. Nay, when health of mind is restored, all that has passed may fade away, and be forgotten entirely or in part. After the delirium of fever, as we know by experience, all the real facts may be lost, and the delirious imaginations only remain. Again, in certain cerebral affections, the memory of certain words may be lost, whilst others remain, &c. &c.

These peculiarities of memory, Dr. Holland thinks, are more connected with decayed states of the hemispheres of the brain, either from pressure, softening, or over-excited or equally from too feeble a circulation in the brain. The powers of memory may be confused or lost from violent exertion or from excessive fatigue.

Dr. Holland enumerates other diseases as affecting this faculty besides those we have already mentioned, such as epilepsy, febrile attacks, influenza, and lastly, old age. On all these points his observations show a wide range of experience, and a very thoughtful observation of phenomena; and we refer the reader with much pleasure to his judicious suggestions for the treatment of these defects of memory. This and the preceding chapter on insanity are two of the four new chapters of the work.

We must pass over the many interesting questions mooted in the chapter "On the Brain considered as a double Organ," as

well as the statements of the principal difficulties involved in phrenology. The latter might be more fully investigated with advantage, and we should be glad to see the pen of Dr. Holland so employed; more especially as we think that sufficient stress has not been laid upon the impossibility of estimating (by external examination) the activity of any organ, and yet the power of an organ and the character of an individual must result from the combination of size and activity. A large organ may be inert, or a small one very active, and so the calculations of phrenology defeated.

The new chapter, "On Instincts and Habits," is a striking investigation into a very difficult subject. What is instinct, and what relation has it to other mental faculties? Dr. Holland throughout compares it with the "reason;" now if by reason we mean the higher or pure reason, we can see no relation and no resemblance except to certain convictions, which in consequence of the intense rapidity of the mental operations producing them are called "intuitions." If we adopt that division of mental faculties with which Coleridge has made the British public familiar, and speak of the reason and understanding, we may then add to these "instinct," as the lower of the three intelligent characteristics. The boundary limits between instinct and understanding ("the faculty judging according to sense") are often very undefined, the two faculties being to a certain extent, and inversely as to their proportion, common to animals and man; while the pure reason is possessed by man alone.

"At one extremity of this great scale we have the blending of intelligence and instinct as we find it in the human being and in the higher animals; at the other the connexion of animal instincts with those of vegetable life."

We quite agree with the author in the essential difference between animal instincts and understanding, although we think the operations of the latter, on account of their amazing quickness, are sometimes confounded with the former. As to the "origin and guidance" of the instinct, Dr. Holland agrees with Sir Isaac Newton in his opinion, "that the Deity Himself is virtually the active and present moving principle in them," not in the pantheistic, but providential sense.

"We cannot conceive the intervention of other agency; but neither are we able to reach the comprehension of that Supreme Power which thus unceasingly sustains the life it has created."

In man himself the limits between instinct and understanding are equally obscure.

"The association of instinct with the higher intellect of man does in truth render all its manifestations more equivocal and obscure. After all we have said on the distinction between the two powers, it is not easy to say where the one ends and the other begins,—to interpret rightly the numerous cases whose automatic and habitual actions, scarcely to be distinguished from instincts, blend themselves with those of reason (understanding) and volition, and in the union accomplish some of the most important functions of life,—to solve the doubts which arise from each class of actions being carried on by the same organic apparatus,—or to define, with reference to the question, those congenital propensities, intellectual and moral, which so strongly mark individual character, and influence the whole course of human life."

Into many of these questions Dr. Holland enters pretty fully, and the difficulties he states fairly, nor have we found reason to differ from his conclusions. One very curious part of the inquiry is as to the agency by which instinctive actions are carried on. By the nervous system,—will be answered at once, certainly; and "if we found certain portions of it invariably connected with certain well-marked instincts, and never present when they are not," the question would be nearly settled. But such is not the case.

"The most distinct knowledge we here possess is indeed of a negative kind. It is certain that neither the central lobes nor cerebellum are necessary, or directly concerned, in instinctive actions, inasmuch as these peculiar organs are confined to the vertebrata, and wholly wanting to larger classes of animals whose instincts are highly developed. Excluding them, the closest analogy we can obtain in seeking for nervous centres of these functions, is probably that of the ganglions appertaining to the organs of special sensation. Comparative anatomy, indeed, concurs with analogy in leading us to conjecture that the ganglionic structure (using the term in its common application) is the nervous apparatus most directly instrumental to instincts. The examination of the nervous system, as we find it in descending the scale of animal life, strengthens this inference, by showing the great development of these parts, their definite arrangement in each species, their prodigious variety in different classes of the animal kingdom. It is true that we reach a point, proximate to the vegetable world, where actions that we must call instinctive are still found in organized beings, unpossessed as far as we can see, of any nervous tissue. Even here, however, we have no proof that nervous matter does not exist in some form, equivalent to the necessities of this low grade of being."

But we must bring our notice to a conclusion. Brief and imperfect as it is, if it induce our readers to study Dr. Holland's work for themselves, it will have conferred a benefit

upon them and have answered our purpose. We can conscientiously assert that it is long since we read a work of equal interest, or one which was more suggestive of important trains of thought. It indicates very extended acquaintance with the labours of others, without losing an original freshness of its own: it is not the work of a professional man merely, but of a well-read, thoughtful philosopher and scholar.

Neuralgia, its various Forms, Pathology, and Treatment; being the Jacksonian Prize Essay of the Royal College of Surgeons for 1850, *with some Additions.* By C. Toogood Downing, M. D., M. R. C. S. London: Churchill. 1851. 8vo. pp. 375.

The giant strides with which the twin sisters, Physiology and Practical Medicine, have advanced during the present century prove that their votaries have not yielded in their efforts for progress to those of any department of general science. It is nevertheless unquestionable that in certain departments of both, their development, which, such is their mutual relation and dependence, is almost always accomplished *pari passu*, has been far from commensurate with that in others. This observation, however, may not be relatively, although positively, applicable. Thus, our knowledge of the nervous system and its diseases, rescued from much of conjecture and uncertainty by the brilliant researches of Bell, followed up by those of Marshall Hall and others, though affording much ground for congratulation when compared with its former state, cannot be regarded as approaching that degree of certainty which may be claimed for other branches of medicine and physiology. It is not difficult to account for this inequality; mainly we find such progress to have been achieved in proportion as, abandoning antiquated speculations for systems of pure induction, the ingenuity of modern times has applied to such investigations the more tangible aids of experiment and observation. Thus each new device of collateral science has been pressed into service, and we have witnessed in our own days difficulties apparently insurmountable yield one after the other before such aids as the microscope, organic chemistry, and the study of statistics afford.

But there is a limit to this applicability. The subject of investigation may be of such a nature that the means at our disposal can reach it but partially, or not all. It may possess intrinsic properties *sui generis*, that analogy can be of

little avail in elucidating, and even accumulated observations may be of necessity defective and incapable of useful generalization, owing to the abstruse or merely objective nature of its phenomena. It is such difficulties as these which the study of the functions of the nervous system and its diseases present, and hence our knowledge of them is comparatively so much in arrear. We possess no certain or demonstrable views of the nature of the vis nervosa, and consequently none, of those morbid manifestations or disturbances of it which, in ignorance of their proximate cause, have been termed neuralgia: or, to speak more generally, of the nature of pain, arising from whatever cause; yet of the existence of such a force we have abundant evidence, and that it is distinct from electricity or any cognate agent with which we are at present acquainted, although the opposite has been maintained, there are many reasons for concluding. Are we, then, to despair of unravelling the laws by which this subtle agency is regulated, or of the nature of their aberrations, which are at present only recognised in their effects? Certainly not. Indications are not wanting that may be likened to the faint glimmering of a light which we hope is ultimately to cheer the toilers in these dark recesses of science. Expecting the full light of that future day, we must now be content with the best attainable *resumé* of our present knowledge, together with such instalments in the shape of additions, as may be afforded by these who venture to cope with subjects in some respects repulsive and unsatisfactory. On this account we conceive that the selection of neuralgia as the subject for the Jacksonian prize essay was judicious. The stimulus of competition has proved of vast use to general as well as to medical science, and the field of investigation in the present instance, although not untrodden, presents, nevertheless, much of the interest that attaches itself to novelty, from the necessity in many cases of striking out new paths of inquiry through the uncertain mazes of the old.

With these feelings and with these allowances we take up the work of Dr. Downing, upon which the conventional seal of superiority has been impressed, as that to which the prize has been awarded. It will be the province of the impartial reviewer, however, to investigate how far he has otherwise deserved his laurels; how far the fact of his book having gained this distinction warrants its being regarded by the profession at large as deserving of their confidence and approbation.

The scope assumed by the author is extensive, embracing,

as it does, the consideration of his subject in all its situations and varieties. He makes three primary divisions of the disorder: the spasmodic, the rheumatic, and the hysterical. Under the first of these he places those affections known as genuine tic dolourcux, a term which he derives from the nervous twitchings of the skin over the affected part, and which he believes to be pathognomonic of the disease, at least so far as to distinguish it from the rheumatic variety. In the description which he gives of it we find nothing new, but there is evidence that he portrays from his own observation of nature, and it is consequently faithful and practical. Under the head of predisposing causes of spasmodic neuralgia we are given a list of most of the excitants to the disorder. Of the influence of age he says, that although it appears to be a general impression amongst authors that genuine neuralgia is a disorder of middle life, possibly no age is exempt from it. He has observed it himself in extreme old age, and whether it attacks very young children we have at least no means of determining in the negative. "A child *may* cry, or even scream with anguish, who can only point to the part, but is incapable of describing the nature of its sufferings." A table of M. Chaponnière's, consisting of 119 cases of the disease, is further quoted, to exhibit the effect of age in its production.

Now here we cannot avoid reiterating a complaint that has been justly urged against inductions drawn from medical statistics, too often compiled as they are without due regard being paid to compensating or conflicting influences. Thus, in the table referred to, while the number of cases occurring between 40 and 50 is given as six times those occurring between 70 and 80, there is no observation to show that allowance has been made for the greatly diminished total number of persons existing, comprised between the latter ages, as compared with the former. To make such a table practically useful, it would be necessary to append to the figures placed under the decennial periods into which the aggregate number of cases collected has been divided, the numbers that would result from an allowance for the diminished or increased total number of persons existing at the periods brought into comparison. Without such an addition, we regard statistics of this nature as of little value; and worse than useless, if relied upon for purposes of induction, being fallacious guides. The other predisposing causes enumerated by the author do not invite special attention. As to the influence of sex, he adduces his personal experience that females are most liable to the disorder. This agrees with

our own, and is in accordance with that of most authorities. Some cases are given to prove hereditary transmission.

The author divides the exciting causes "into the local and the distant, or those which act directly on the nerve itself, and such as influence it by sympathy with a distant organ." Of the latter class, when speaking of derangement of the alimentary canal as one, the testimonies of Abernethy, Bell, Sir B. Brodie, and other distinguished names, are cited in support of it; and it is an opinion that we think will not be contravened in this country, although in France scarcely admitted. Under the heads of temperature, osseous deposits, and disease of bone, we find nothing worthy of comment, except that he objects, and we think justly, to the dogma of Sir H. Halford, that facial neuralgia is always connected with osseous disease. Disease of the brain is next considered; and of this he observes, that not only the morbid conditions of this organ found after death in certain neuralgic patients, but the osseous deposits within the cranium, have more probably resulted from the continued neuralgic irritation than the latter from the former; and he adds:

"I would, therefore, suggest, that possibly those extensive ossific deposits, and some organic lesions noticed in the cranium, may have originated from long-continued irritation of the sentient nerves. They may be regarded as signs and evidences after death of a previous local nervous excitement. The same kind of reasoning is now applied with plausibility to spinal irritation. The opinion is now held by many, that this is not in general an idiopathic disorder of the medulla, but that it is merely the consequence, and the index, as it were, of morbid action in other regions."

We are disposed to admit the occasional action of peripheral nervous derangement in producing centric disturbance, or even disorganization, as regards either the brain or spinal marrow; in the case of the latter, being borne out, as the author states, by what appears to occur in certain cases of spinal irritation. We cannot so easily subscribe, however, to his explanation of the production of osseous tumours within the cranium. If at all adopted, the same reasoning would, we conceive, be applicable to the bones forming the spinal theca. But we must go a step farther: other diseases of bones besides their abnormal nutrition, as the author seems to admit when he speaks of "some organic lesions," must, by the same rule, be attributed to a similar cause, and why not caries? Yet we are not prepared to hear it alleged that the disease known as Pott's curvature can ever be traced to this origin.

Dr. M'Culloch's views of the common origin of malaria and intermittent fever the author justly considers entitled to respect. The arguments and cases adduced by that gentleman in his work on the above subjects appear to be in themselves conclusive, and we think general experience has, in the tolerably long interval since it was published, confirmed his painstaking observations.

After glancing at some of the less important exciting causes of the disorder, the question is raised as to which of all that have been enumerated most frequently comes into operation, and of these, after stating the difficulty of answering it satisfactorily, owing to the influences of local causes in each particular form he says, "cold may be considered as the chief excitant, closely followed by dyspeptic irritation and malaria." He adds, however, that in the great majority of cases, no probable cause can be assigned for its first onset, in proof of which a table by Valleix is cited, from which it appears that out of 67 cases, in 46 instances the disease occurred without apparent cause.

The local causes of neuralgia are dismissed rather summarily. Dr. Downing regards neuralgic pains, when persistently located in a particular nerve or spot, as "due to some local determinant cause." This is, probably, quite true, but we think we can discover that the theory is here put forward for the purpose of adapting it to the practice most relied on by the author in the treatment of local neuralgia,—the application of local means. This section contains nothing further deserving of comment, and we pass to his account of the proximate cause. It is here that the great difficulties of the subject commence. Here it is that our ignorance of the nature of the vis nervosa, or that secret and inexplicable agency upon which all animal sensation and motion are dependent, presents a barrier to any signal advance in this direction, which in the present state of our knowledge it is hopeless to expect to surmount. It is true that attempts have been sedulously made to connect the symptoms during life with appearances observed in the affected nerve after death, and in a few instances with apparent success. But even supposing this difficulty satisfactorily solved, and that we could with certainty discriminate the morbid causation, so far as alterations of structure (if invariable) are concerned, we would be still equally in the dark as to the immediate cause of the pain itself. Our knowledge would be on a par with that which we possess about the physical properties of certain drugs, by which we at once discriminate them from each other, but are altogether ignorant as to how it happens

that one of them purges, another vomits, a third produces perspiration, or a fourth causes muscular spasms. And this last-named property possessed by certain substances of producing nervous disturbance, as manifested by pain and spasm, independently of any mechanical explanation that could be offered of their occurrence, goes, we think, some way to show that neuralgia may exist independently of all organic change, and that possibly in those cases where such changes have been observed, effects, as not unfrequently happens, have been mistaken for causes.

The notion formerly prevalent, that the disease was attributable to an acrid matter effused around the nerve, by which it was irritated, or even physically compressed, is not, as the author observes, even yet quite exploded. He is opposed to the idea of its inflammatory origin, as maintained by Dr. Rowland, and the reasons he gives for entertaining a contrary opinion appear to be sufficient. Are the affected nerves in a state of hyperemia during a neuralgic paroxysm? "This opinion," says Dr. Downing, "has arisen in consequence of the skin over the affected nerve being sometimes observed to change into a deep red colour during the attacks." This he explains by the well-known axiom—"Ubi stimulus, ibi fluxus." The evidence obtained from post-mortem examinations of the condition of nerves that had been affected, as he truly observes, is conflicting and unsatisfactory. The opportunities for them are necessarily infrequent, and the results of those that have been made, as proved by several quoted by him, so opposite, as to leave no grounds for reliable conclusions. It is sufficiently proved, however, that severe neuralgia may exist for years, without any change appreciable to the unaided vision being discoverable after death. But while, with Dr. Downing, we regret that the microscope has been so rarely called in to aid in the search after structural alterations, we can only pardon so great an omission in a systematic treatise of the extent and pretensions of that before us, as the want of its most valuable evidence, on the supposition that no opportunity presented for its employment.

The author alludes to the exploded idea of Fothergill, that neuralgia is dependent on the cancerous diathesis, and the equally erroneous one, that it is attributable to a syphilitic taint, but he grants that either of them may be instrumental in its production. There is another diathesis, however,—the gouty,—of which he omits all mention, both here and elsewhere, as a special and perhaps not infrequent cause of the disease. A very interesting and well-marked case of neuralgia will be

found in Dr. Graves' Clinical Medicine, in which it was ascribed to this source by that acute observer[a]. The hypothesis of Dr. M'Culloch is again considered,—a long quotation, in which his views are ably supported, being given from his work. But, as Dr. Downing observes, "even by conceding that the diatheses in ague and neuralgia are identical, which in some cases is very probable, we arrive no nearer the solution of the chief difficulty as to the *nature* of this diathesis." We do not see the necessity of subdividing the consideration of this subject, and for the very reason conveyed in the sentence just quoted, namely, our ignorance of the conditions generated in the system by the action of malaria, and consequently of this as a proximate cause of neuralgia. We should have preferred finding all that occurs here on the subject appended to the remarks under the head of exciting causes, to which in fact it properly belongs.

The next point considered, and one that comes more appropriately under the present heading, is the probability of motive changes occurring in the nerves themselves, in connexion with the neuralgic paroxysms. This idea, although stated by the author to have been an independent conception of his own, he afterwards found to be supported by the opinion of Sauvages, in a work published exactly a century ago, and also in the present day by the high authority of Swan, who "is decidedly of opinion that neuralgia frequently depends upon spasm of the nerve." We think, however, it will be better to allow the author to explain his own views on this subject, which are concisely presented in the following paragraph:—

"It cannot be supposed that the neurile matter itself is possessed of contractility. The power must reside, if at all, in the neurilemma, the structureless membrane enclosing it, identical with the sarcolemma of unstriped muscular fibre. This tissue enveloping each fibrilla, must constitute a considerable part of the bulk of each nerve. Its properties have been little investigated. It is known to possess elasticity. Contractility is the most universal principle of the living system. It resides in most of the tissues, and is capable of being roused into activity by appropriate stimuli. It may exist in the neurilemma, although ordinarily quiescent. No great liberty is taken, therefore, in supposing that this tissue is susceptible of extraordinary excitement under disease, and by compressing and extending the neurile matter giving rise to the severest local pains."

Against this, which, like every pure hypothesis not in the

[a] Edition by Neligan, vol. i. p. 469.

nature of things infeasible, is deserving of attention so long as it remains disproved, we have nothing to object. The opinion of Mr. Swan, from the great attention he has bestowed on the subject of the nervous system, is deserving of much attention; but he does not appear to have described the *modus operandi* of the spasm or contraction referred to, contenting himself with stating his belief in its occurrence.

Under the head of morbid associations, Dr. Downing particularly dwells on those of muscular spasm and paralysis. But we think he has not done justice to this part of his subject; several complications of neuralgia are not even mentioned, as, for instance, epilepsy, of which Dr. Copland has given a remarkable example in his Dictionary of Medicine, in which the epileptic paroxysms were preceded by those of the neuralgic pain, and appeared to be dependent on them. Diagnosis is also dismissed summarily, but the author states that the subject will be interwoven with other parts of the book when treating of the special forms of neuralgia.

The treatment of neuralgia is divided by Dr. Downing into that by systemic or general, and local remedies; the former are first considered. With this subject we do not mean to occupy the reader long. Most of the remedies already well known to the profession are passed in review, and their merits compared. The well-established one, carbonate of iron, he considers should always be tried where the case admits of it; and he agrees with Dr. Hunt, that arsenic should be pushed until symptoms of poisoning appear, when its specific effect is only to be looked for. We must add, however, that such a proceeding should be managed with the utmost caution, lest, in the words of the old adage, "the cure prove worse than the disease," an event, in careless hands, not unlikely to occur. Of opium and its preparations he well observes, they do not seem to exert their usual soothing effects in this disease: the large doses of them that will be borne testify this. As a sedative, he prefers the cannabis indica, when genuine, to all others, with the exception of morphia. With regard to Dr. Downing's remarks upon local remedies we have the same observations to make as of the systemic; with the exception of a "new method of treatment" adopted by him, as he says, with much success, and separately discussed as such—there is little to detain us. As to epispastic agents, he considers their efficacy to be in proportion to their rapidity of action; he therefore prefers the liquor to the emplastrum lyttæ. He is not favourable to the use of moxas. Of the vegetable alkaloids exhibited in the form of ointment, including aconitina,

which has been so much vaunted by some authorities, he says but little. But of the endermic methods of our French neighbours he thinks highly in certain cases of localized neuralgia. Of chloroform he observes, "No mode of application has hitherto been hit upon that is worthy of confidence. In the only cases where it has been employed that I have heard of, the result has been both trifling and ephemeral." And when previously speaking of its inhalation in neuralgia, he says: "But its effects are quite transient. As soon as the stupor is over, the agony returns with the same violence as before." We must, however, refer Dr. Downing to the tenth volume of our present series, in which he will find extracted from the "Bulletin Thérapeutique" for April, 1850,—and therefore available a considerable time previous to the appearance of his work,—a paper on the use of chloroform in certain affections, by the distinguished physician M. Devergie, in which he says, speaking of its efficacy in neuralgic pains: "Frictions with an ointment chiefly composed of chloroform, in the proportion of four parts to thirty of lard, relieve such pains quickly, and with much certainty." He also gives the particulars of the case of a lady who had been for years subject to the most intense facial neuralgia, who was permanently cured apparently by having been placed a single time under the influence of this agent. Galvanism he regards with ourselves as a very uncertain remedy. To Dr. Corrigan's treatment by the application of a heated button we hardly think he does justice: we have found it in certain cases highly useful. "Contrivances for the application of warm air, either dry or combined with moisture, have in addition been recommended." But of this we shall have more to say hereafter. As might be expected, the plan of treatment, by the local application of benumbing cold, adopted "by a gentleman of Brighton,"—his name, Dr. James Arnott, is a deficiency which we can supply,—finds little favour at the hands of Dr. Downing; the local treatment considered by him most reliable, and claimed as novel, being, as we shall see, the very opposite. Yet we find immediately afterwards that the author endeavours to show from physiological considerations founded on quotations from the writings of Todd and Bowman, and Dr. Golding Bird, that stimulating and sedative agents may produce like effects on the nerves—"temporary loss of polar or erectile power, but in an opposite manner." Heat and cold may, perhaps, be both regarded as sedatives when raised or lowered respectively to certain points. We do not mean to advocate the treatment of neuralgia by cold, but to place the matter impartially under the notice of the reader. Section of the nerve is

justly regarded by the author as the last resource of art. The evidence adduced proves the results of the operation to be discouraging on the whole. One valuable resource we miss entirely in our author's list of remedies, and from our own knowledge of its efficacy in suitable cases, we must regard it as a serious omission, we mean acupuncture; neither here nor elsewhere in his book, which we have read carefully through, do we recollect it to be so much as alluded to.

We have at length arrived at that portion of Dr. Downing's work which lays most claim to originality; and as it purports to be of practical interest, that to which we beg particular attention. "The New Method of Treatment," as it is headed, suggested itself to the author, he tells us, some years ago, in consequence of the more or less total failure in a large proportion of cases of all the agents he had just enumerated. The result of his investigations was subsequently published in a pamphlet, and favourably received by the profession. The new treatment consisted in the application of warm medicated vapour to the affected part by means of an instrument he had invented for the purpose. To this pamphlet the reader is referred for a full description and engraving of the instrument. This course, which is perhaps modestly intended by the author, involves, we think, a considerable omission in the work before us, especially as we afterwards find the greatest stress laid upon the use of the apparatus. It consists, we are informed, however, of "a cylinder for igniting the vegetable matter, bellows for maintaining a current of air through the burning materials, tubes and cones for directing and concentrating the stream."

The mode of using the instrument will be easily conceived. The materials for combustion, which consist of the "leaves, slender stalks, and seeds of plants," having been placed in the cylinder and ignited, a current of air, varying in intensity at the pleasure of the operator, is directed through the mass, and thence by means of a tube and cone, or nozzle, according to the part affected, upon the surface or into a cavity of the body. The plants "chiefly employed have been various mixtures of belladonna, henbane, cannabis indica, tobacco, hops, aconite, stramonium, hemlock, digitalis, &c." That the heat of the current of air will be capable of graduation by the greater or less energy with which the bellows is used will be readily understood. Whether the chief therapeutic action of the application is to be attributed to this heated air, and not to the specific effect of the herbs employed, is a consideration discussed at some length. We have seen from a quotation from the author himself that the use of hot air and unmedi-

cated vapour is not a new idea. It has been objected, he says, that the virtues of the herbs must be completely destroyed by the act of combustion, and also that, even supposing the vapour arising from them to preserve their characteristic power when otherwise applied,—the skin would present a complete obstacle to its effects on the affected nerves. Against both objections he urges some plausible reasons for a contrary opinion, which we do not mean to contravene hastily. But when we come presently to scrutinize the cases adduced as treated by the author's "new method," we shall have occasion to ask ourselves whether we consider his own mind to have been made up upon these important points.

A large portion of the book is taken up with details of cases illustrative of the disease in most of its forms and localities. They amount to seventy-three in all; we have been at the pains to count them, the numbering of them, which is a useful aid for the purpose of reference, having been omitted by the author. Some of these are of very considerable length, but we object less to this than to a useless repetition, as for instance, as regards tic douloureux, of cases not only so similar to those preceding them, but of such ordinary occurrence as to present no more practical interest than the details of a number of cases of toothache would of that form of neuralgic pain. Should a second edition of the work be called for, we advise considerable retrenchment in this part. We would also suggest to the author the preparation of an analysis of the cases given with the kind of treatment adopted, together with the results of the latter. Many of the cases recorded appear to have received no treatment, at least none is mentioned, and in a large proportion it would be difficult to assign the curative efficacy that occurred in any one in particular. Then the mode in which Dr. Downing expresses himself is often so loose as to leave the exact treatment adopted altogether a matter of conjecture: for example, while he constantly uses the expression: "warm sedative vapour" to express treatment by means of the instrument which he has elsewhere named the aneuralgicon, although not where the description of it is given; he, in other instances, contents himself by stating that "local measures of a sedative nature," or "soothing applications," were adopted, by which might be understood belladonna or opium plasters, just as well as his peculiar plan of treatment.

In a few cases in which he has been more explicit, however, genuine tic douloureux of long standing, and in which various plans of treatment had been employed and abandoned as useless, appears to have been permanently eradicated by the

use of the instrument. These alone entitle Dr. Downing to the thanks of the profession, as it would appear that he has added to our list of remedial agents, all of which are too often futile in this intractable disease, one that is occasionally attended with great advantage. Here, however, we must remark that there is either great want of precision in the description of it given with the cases, or, as we have already hinted, that the author, notwithstanding his arguments in favour of the specific action of the medicated vapour through the medium of the skin, and his enumeration of the different herbs he was in the habit of employing in its production,—the relative degrees of merit of each of which we expected to find compared,—had misgivings as to the specific powers of any of them, and referred the real efficacy, as had been done at the Medical Society of London, when he brought his cases under its notice, to the warm air and not to the influence of the agents employed for combustion[a]. That these ideas would naturally occur to the reader it will be only necessary to mention, that it is not until he arrives at the 326th page, and the sixty-sixth case that he is informed, for the first time, what the aneuralgicon had been charged with during its application,—henbane and belladonna being the plants employed. Nor is the circumstance again alluded to except in the seventy-third, or last case, and in the last page of the book but one, where the expression, "varying the herbs according to circumstances" faintly reminds him that the author had not altogether lost sight of this, as he might expect to find, essential part of the "new method of treatment," involving as it does that part of it for which the claim of novelty could be most safely urged.

Interwoven with this large collection of cases, and introductory to each special class, under most of which some of them are arranged, the author has appended observations upon their general history and diagnosis; these need not detain us long, but we must offer a few remarks upon them.

As to the vexed question, whether the portio dura of the seventh pair is ever the seat of neuralgia, the author feels disposed to answer it affirmatively. In this he is supported by the opinion of Dr. Copland and some other physicians of eminence, and we must here beg to tell him that Dr. Graves, of Dublin, in his work already quoted, has described an affection of this nerve, named by him "Bell's spasms of the portio dura," in terms so nearly resembling those given by Dr. Downing as original, when he speaks of the characteristic symptoms of its

[a] Lancet, January 13, 1849, p. 45.

neuralgia, that we cannot avoid taxing him either with want of candour with respect to the source from which the observations, although supported by cases of his own, were first obtained, or with culpable ignorance of their having been previously made by the high authority in question[a]. The descriptions of neuralgia of the female breast and of the testis are entirely quotations from Sir A. Cooper, as is that of the joints from Sir B. Brodie's work. That of the ovary,—of which one case is given, and of which it is stated, "great benefit was here derived from quinine, and the local application of warm sedative vapour; nothing else appeared to influence the complaint;"—we refer to in order to direct the attention of the author to the local treatment adopted by Dr. Churchill, which he will find described in his paper on Ovarian Irritation, published in the twelfth volume of our present series, and of the value of which we can ourselves speak from experience with perfect confidence. The last section, and one that he tells us in his Preface is "quite novel," is devoted to traumatic neuralgia, a name which he applies to those neuralgic affections resulting from local injuries, whether the effect of accident or of surgical operations. The causes that give rise to it he divides into,—"1st, The presence of a foreign body in the substance of the nerve. 2nd, The entire or partial division of a nerve. 3rd, Pressure upon a nerve." Each of these divisions of the subject is well discussed and illustrated by quotations from authors, but we have not been able to discover any observations of sufficient originality to induce us to dwell upon it. He claims, however, to have been the first who has treated the subject in a separate manner.

In conclusion, we would observe that, notwithstanding our strictures upon Dr. Downing's work, we have not lost sight of the difficulties of the subject arising from the causes which we noticed at the commencement of our review. We do not, nevertheless, hesitate to say that we regard it as a clever and comprehensive treatise, and one that may be read with interest and advantage.

[a] *Op. cit.* vol. i. p. 570.

Illustrazione di tutti gli Strumenti Chirurgici scavati in Ercolano e in Pompei, e che ora conservansi nel R. Museo Borbonico di Napoli, compresa in Sette Memorie, lette all' Accademia Ercolanese. Dal Cav. Benedetto Vulpes, &c. &c. Napoli, dalla Stamperia Reale. 1847. 4to. pp. 94, with seven Plates.

Description of all the Surgical Instruments discovered in Herculaneum and Pompeii, and now preserved in the Royal Bourbon Museum of Naples; comprised in Seven Memoirs, read before the Herculanean Academy. By the Cavalier Benedetto Vulpes, &c. &c.

A Layard has not as yet, it is true, dug up the remains of a railway train with its first, second, and third class carriages, nor has a Belzoni lighted on the *débris* of an electric telegraph, but surely a strange foreshadowing of the latter vast invention existed in the human mind when Strada, more than two hundred years ago, in prophetic language wrote of "needles affixed to dial-plates inscribed with the four and twenty letters of the alphabet, which, under the influence of the loadstone, by their sympathetic movements enabled two friends, though several hundred miles asunder, to converse together across a whole continent, and to convey their thoughts to one another in an instant, over cities or mountains, seas or deserts"[a].

The discovery of various surgical instruments among the ruins of Herculaneum and Pompeii, many of them closely resembling those in use at the present day, and some of which appear even to have been forgotten for centuries, and to have been re-invented in comparatively modern times, affords additional illustration of the truth of the saying, that "there is no new thing under the sun." To illustrate and describe these very interesting relics of antiquity is the object of Professor Vulpes' work, nor can we better introduce the subject, or more briefly explain the nature of the discoveries which have been made, than by presenting our readers with a translation of the learned author's preface, and of the catalogue appended to it.

"From the middle of the last century, when Charles III. began to disinter the precious monuments of the two ancient cities of Herculaneum and Pompeii, to the present time, numerous surgical instruments of bronze, and some of iron, have amongst other things been found, all of which have been collected and preserved in the Royal Bourbon Museum. Although many strangers, in the published accounts of their travels in Naples, and also some Neapolitan

[a] Spectator, No. 241, Thursday, December 6, 1711.

writers, have spoken of these instruments, nothing appeared in the Transactions of the Royal Herculanean Academy, which was occupied in the illustration of so many other ancient remains preserved in the Royal Museum, on this archæologico-chirurgical subject. Having been on the 27th January, 1846, appointed a corresponding member of this Royal Academy, I felt myself called on to undertake the task, especially as I had already described some of them in the fourteenth volume of the 'Real Museo Borbonico' (Naples, 1846). Accordingly, between March, 1846, and the 27th January, 1847, I presented seven memoirs to the Academy, containing a description of all the surgical instruments of bronze and iron. The work having been approved, in its professional aspect, by the Royal Academy of Sciences[a], was adopted for its archæology by the Herculanean Academy [Accademia Ercolanese], and published in the seventh volume of their Memoirs.

"It may here be mentioned that many of these instruments were first found in Herculaneum, and Bayardi has given an enumeration of them in his 'Catalogue of the Ancient Remains disinterred in the newly discovered City of Herculaneum,' printed in the year 1754. Subsequently, forty were found at Pompeii, in a house situated at the head of the Corso, as we enter it by the Herculaneum gate from the Street of the Sepulchres, and on the left, directly before the house of the Emperor Claudius. It has been called the Surgeon's House [Casa del Cerusico], and by others the School of Surgery. But as it differs in no respect from other private houses, the opinion of those who maintain that it was not a school of surgery, but the house of a surgeon, or perhaps the house of a private individual in which a surgeon lodged, is more probably correct. Romanelli, in his journey to Pompeii, published in 1817, stated that five other surgical instruments were found in the midst of some cooking implements, in the shop of a vender of oil, situated in the neighbourhood of the house of Pansa and Marcellus[b]. The great uterine speculum was found about 1818–19.

"All the instruments amount to about two hundred; many of these are, however, so similar as to constitute mere repetitions of one another. I exclude from this enumeration all broken instruments, and some of which the handles alone remain, since almost all of these are similar to other specimens better preserved, and described by me. The uses to which these instruments were designed by the ancients are almost always evident on simple inspection. I have, however, thought it necessary to corroborate the opinions which the appearance of the instruments may suggest as to their uses by the authority of the writers who lived about the epoch in which the eruption of Vesuvius buried the celebrated cities of Herculaneum and Pompeii. With this view I very frequently refer in the

[a] See the Appendix to the fifth volume of the "Rendiconto de' lavori della Reale Accademia delle Scienze di Napoli per l'Anno 1847," fasc. xxx. p. 432.

[b] See "Romanelli viaggio a Pompei." Parte prima, pag. 104. Second Ed., 1817.

course of my work to the writings of Hippocrates, Galen, Aetius, Paulus Æginetus, Cornelius Celsus, &c. From the medical works of the last named in particular, who, to use the words of Scultetus, was most careful in the description of surgical instruments[a], I have derived the greatest assistance. If these instruments have been much improved by the perfection to which in our time the art of Podalirius and of Machaon has gradually attained, an acquaintance with them will at least contribute to the history of the art, and may be useful in explaining some parts of the writings of ancient authors which would otherwise be obscure and unintelligible, and they also give us the opportunity of seeing some instruments which are merely mentioned by the old writers."

The author prefixes to his work the following enumeration of all the surgical instruments which were found in the ruined cities:—

Forceps,	With curved claws,	1	
	With half spoons,	1	
		—	2
Canulas,	To draw off the fluid from the abdomen in ascites,	2	
	For the same purpose, and for introducing medicated liquids into any cavity,	3	
		—	5
Probes,	Spatulated at one end and ovoidal at the other (*spatomeli*),	18	
	Ovoidal at one end, and concave at the other (*ciatischi*),	21	
	Ovoidal at both ends (*dipireni*),	2	
	Ovoidal at one end, and terminated by a flattened button at the other,	1	
	Pointed at one extremity, and spatulated at the other,	1	
	Very long, and with one extremity pointed,	2	
		—	45
Fourchette for cutting the frænum of the tongue,		1—	1
Double-curved catheter,		1—	1
Speculums,	Speculum magnum matricis,	1	
	Speculum ani,	1	
		—	2
Forceps,	Anatomical,	15	
	Tenaculum-shaped,	1	
	Depilatory,	72	
		—	88

[a] Sculteti Armamentarium Chirurgicum, P. I. pag. 7. Francofurti, 1666.

Tenaculums . .	Simple, ...	13	
	With the extremity of the handle spatulated, ...	1	
		—	14
Trident for cautery, ...		1—	1
Small spatula to remove the eschar formed by cauterization, ...		1—	1
Silver-bladed lancet, ...		1—	1
Little spoon for examining the blood removed from a vein,		1—	1
Cupping Instruments,	Conoidal, ...	8	
	Spheroidal, ...	5	
		—	13
Instrument for bleeding horses, ...		1—	1
Levers, ...		2—	2
Iron Instruments,	Bistouries, ...	13	
	Scalpels, with bronze handles, . .	2	
	Scalpels, entirely of iron, ...	1	
	Hooks, with bronze handles, . . .	2	
	Cautery iron, ...	1	
Similar instruments of bronze for the same purpose, .		2	
		—	21
	Total, . . .		199

The author in his opening chapter discusses the uses to which the forceps with curved claws, the first instrument in the foregoing catalogue, may have been applied. Having stated the general opinion to be that it was employed in the extraction of loose teeth, he proceeds to show that it was more probably used for the removal of fragments of bone, broken pieces of darts, &c., from small cavities, and that it may also have served for seizing arteries preparatory to the application of a ligature. In support of this latter view he quotes Celsus,[a] who proposes the employment of ligature in cases of hemorrhage from wounds in which stypties have failed, directing that a double ligature shall be applied to the vessels transmitting the blood—"*Quod si illa* (medicamenta) *quoque profluvio vincuntur*, VENÆ *quæ sanguinem fundunt* APPREHENDENDÆ *circaque id quod ictum est, duobus locis deligandæ.*" Professor Vulpes has quoted sufficient to prove that such a forceps may have been used as auxiliary to the application of ligatures on arteries; a perusal of the remainder of the sentence in the original will show that Celsus' intention was

[a] Lib. V. cap. xxvi. n. 21.

that the bleeding vessel should be divided between the two ligatures, so as to cause it to retract, while the mouths of the divided vessel should, nevertheless, be closed. The author also points out that the indentations on the extremities of the claws of the forceps are not like those of a file, composed of crossed lines, which might wound or lacerate the artery, but of long, triangular prisms, the prominent angle of which is very obtuse, and therefore incapable of injuring the parts seized on by the instrument.

His remarks on the ligature of arteries, as known to the ancients, are interesting; and we shall therefore, although the subject is treated of in most of the systematic works on surgery, give them at length.

"And here it will be useful to bear in mind that the ligature of arteries as a mode of restraining hemorrhage was known to the Greeks, from whose stores of knowledge Celsus has so largely drawn. In fact, from a Greek inscription dug up in the neighbourhood of Athens, it is evident that Evelpistus the surgeon used this plan. This Evelpistus, who probably migrated from Greece to Rome, seems to have been the person mentioned by Celsus as among the professors who were above mediocrity, and who not only improved some things in surgery, but added a little to the art. Besides, in the famous collection of books of the old Greek surgeons made by Niceta, and which are now preserved in the Laurentian Library at Florence, among the directions given by Archigenes on the amputatation of limbs we read, 'therefore the vessels leading to the part to be cut off are to be tied with a string or stitched.' This Archigenes was the writer of many works which have been lost, some fragments of which are to be found in the writings of Galen, Celius Aurelianus, Aetius, and Paulus Æginetus. It is he who, quoted by Aetius, mentions the speculum uteri, which he called 'dioptra'[a]. He was so renowned that Juvenal in his Satires, to indicate a physician, called him by antonomasia Archigenes, as in the fourteenth Satire, where he facetiously tells an old man who was obliged to guard himself from the poison of a covetous heir—

> "'Ocyus Archigenem quære, atque eme quod Mithridates
> Composuit, si vis aliam decerpere ficum
> Atque alias tractare rosas.'

"But to return to the ligature of arteries. This mode, forgotten, as it were, in the barbarous ages, in which it was usual to arrest the hemorrhage after amputation by the application of the actual cautery or by dipping the stump in boiling pitch, sprang into use again with the resurrection of science in the sixteenth century. Ambrose Paré, hailed as the restorer of surgery in France, abhor-

[a] Actii Tetrabiblon, iv. serm. 4. De uteri exulceratione Archigenis, cap. lxxxviii.

ring the above most cruel means, was the first to use the ligature of arteries as it was practised at Naples from 1788 by Cavaliere Santoro in one of our great hospitals. Paré, in explaining the mode of performing such an operation, gives the figure of a forceps for seizing the artery to be tied. Who would believe it? The instrument of the illustrious French surgeon strongly resembles, especially in the claws, the forceps preserved in the Royal Museum."

In the second chapter we have a description of a forceps, the blades of which are made to terminate in half-spoons, the margins of the latter being so dentated as to dovetail with one another, and to form when joined a bowl-like concavity. The author suggests that this instrument may have been designed for passing into a long internal cavity, such as the nostril, for the purpose of seizing excrescences, and retaining them so as to admit of their extirpation or excision. A canula is next described, which he supposes was employed for drawing off the fluid of ascites. It is furnished with a transverse circular plate, dividing it into two unequal parts, apparently for the purpose of preventing the canula entering too far into the abdomen. The chapter concludes with a description of three canulæ, which, in the author's opinion, may have been used for giving exit to deep-seated pus, or for the introduction of medicated fluids. In support of the latter view, he quotes a passage from Hippocrates[a], in which he speaks of a canula for passing remedies along the vagina to the cervix uteri.

The third chapter contains dissertations on the several species of probes enumerated in the catalogue. Towards the close the author describes a catheter of bronze contained in the collection of ancient surgical instruments in the Royal Bourbon Museum. This instrument is ten inches long, three lines in diameter, and is doubly curved. In allusion to the double curve, the author refers to the Isagogic Books of Galen, and especially to that attributed to this writer under the title "Introductio seu Medicus," where, in Chapter xix., "De Chirurgiæ Speciebus," mention is made of a catheter in the form of the letter S. Most ancient writers, however, seem to speak of catheters as having but a single curve—

"Before the middle of the last century," observes Professor Vulpes, "the distinguished surgeon, Jean Louis Petit, perceiving that the single-curved catheter, when left permanently in the urethra, excited inflammation and gangrene in the membranous part of this passage, invented a double-curved catheter, nearly resembling the letter S. Petit might," he continues, "have obtained

[a] De Morbis Mulierum, lib. ii.

the idea of such an instrument from the works of Galen, but he could not have heard of its discovery in the excavations at Herculaneum, since Bayardi did not publish his catalogue in Naples until the year 1754, and Petit died on the banks of the Seine on the 7th of April, 1750, at the age of 77."

In his next chapter our author passes to the description of the speculum magnum matricis and of the speculum ani. The former instrument was known to surgeons from a very early period, but subsequently fell into disuse. Hippocrates has described a speculum for dilating the anus and observing the condition of hemorrhoids, We find from Aetius that Archigenes made use of his "dioptrum" for dilating the vagina and examining ulcers of the uterus. Paulus Æginetus speaks of a small dioptrum for dilating the anus. These instruments were, however, more properly speaking, dilators than speculums. The uterine speculum, for example, was very different from that invented in the commencement of the present century by Signor Recamier, and successively improved by others, consisting of a metallic tube, highly polished on its internal surface, so as to reflect the light on the cervix uteri, and thus exhibit its condition. The ancient instrument was only indirectly a speculum; by dilating the vagina it allowed the cervix uteri to be seen. It is composed of three blades, each convex on its outer surface, but internally presenting a triangular prism, so that, when brought into contact, they touched one another by six surfaces. By the union of the three blades, the margins of which fit one another perfectly, a sort of cylinder, half-an-inch in diameter, is formed, and at the distance of six and a half lines from the top is a swelling eight lines wide, causing the cylinder to terminate in an olivary form. The instrument is so constructed that by turning a screw attached to it from right to left, the plates can be approximated to admit of its easy introduction into the vagina, and by subsequently turning it in the opposite direction from left to right, they can be separated so as to include, when at their greatest divergence, a circular space of three inches and eleven lines. By means of this arrangement, the observer is enabled to inspect the organic changes of the os and cervix uteri. Before removing the instrument, the screw must, of course, be again turned from right to left. This is, we may add, almost exactly the principle of the dilator of Weiss' modern three-branched speculum vaginæ.

The speculum ani is very simple, and on the principle of the instruments at present used for dilating the nostrils and ears.

The fifth chapter or "memoir," is occupied with descriptions

of the three varieties of forceps to be found in the Museum, of the little hooks or tenaculums, of the suture needles, and of the trident or three-toothed fork, which Signor Vulpes supposes to have been used for producing eschars on the abdominal parietes in diseases of the spleen, a view which he supports by quoting Paulus Æginetus, who describes such a practice, and mentions that Marcellus used a trident for this purpose:

"This practice," adds the author, "of cauterizing the abdomen in affections of the spleen, was also in vogue among the Arabians, thus, Haly-Abbas, 9 pract. c. 76,—'Ferro bifurcato splenem adurit;' and Albucas, lib. i. chir. c. 31,—'Pro splene bidentatis, et tridentatis setonibus utitur.' Cornelius Celsus formed the eschars with potential caustics instead of the actual cautery."

Attached by a string to the trident is a small bronze spatula, part of the handle of which is elegantly turned, which the author thinks may have been used to remove the eschars formed by the trident.

The forceps represented in Plate V., figs. 5 and 6, appear to us to be the prototypes of Asellini's forceps, instruments which practical surgeons know to be of great use in assisting them to pick up the open extremities of bleeding arteries which present themselves in deep and narrow wounds, such as are made in the operation of lithotomy, and in which the ordinary tenaculum is comparatively useless.

The only silver instrument in the collection is a lancet, the blade of which is of this metal, and the handle of bronze. It is regarded by Professor Vulpes as the phlebotome of the ancients, and is accompanied by a small spoon of bronze, which he supposes to have been used in examining the blood when drawn from the vein. The practice of blood-letting is very ancient, being frequently spoken of by Hippocrates and Galen; and in the time of Celsus it was probably too indiscriminately employed, for he says—"Sanguinem incisa vena mitti, novum non est: sed nullum pæne morbum esse, in quo non mittatur, novum est"[a]. Celsus directs the operator to observe the appearance of the blood which is first drawn, in order to judge whether the operation is advisable or not; and in the latter case to stop it at once.

The ancients also abstracted blood by scarification and cupping. The cupping instruments were made of horn, glass, or bronze, the latter being most commonly employed. Thirteen constructed of this material were found in the excavations at Herculaneum and Pompeii; of these the author delineates two.

[a] Lib. II. cap. x.

Dry cupping was likewise in use among the ancients; it was supposed that air (spiritus) was removed in this operation[a]. Exhaustion was accomplished by means of suction, when the instruments of horn were employed, and of burning flax when those of bronze.

The collection also contains an instrument of iron for bleeding horses. Hippocrates, in the third chapter of a book, "De Equorum Medicinâ," speaks of this operation.

The author concludes his sixth memoir with a description of the lever used for raising depressed portions of bone, which closely resembles that in use at the present day.

The seventh and last memoir is occupied with a description of the surgical instruments made of iron which have been found in Herculaneum and Pompeii. These consist of thirteen bistouries, three scalpels, and three hooks, of the largest of which latter the author gives a drawing; he supposes it to be the hook spoken of by Celsus as employed in the extraction of a dead fœtus, and which he directs to be inserted for this purpose into the "eye, ear, mouth, or sometimes even the forehead"[b]. The last instruments described in the volume are, some of iron, and others of bronze, for the purpose of applying the actual cautery.

The hardness of the Herculanean and Pompeian bronze, a compound of copper and tin, is remarkable, and allowed the construction of sharp and cutting instruments of this alloy.

An examination of the beautiful engravings with which the volume is illustrated shows that the instruments were admirably constructed, and many of them highly ornamented with elegant chasing.

We have thus endeavoured to give our readers a general idea of a book we have ourselves perused with very great pleasure, not only on account of the interesting and instructive nature of its subject, but also of the large amount of classical lore with which Professor Vulpes has enriched his pages. In so doing, we have not followed the author in his minute descriptions of the several instruments spoken of by him: for such full details we must refer the reader to the work itself. Our principal object has been to show the progress surgery had made in the days of those ill-fated cities, and to state briefly the principles on which some of the instruments found in them were constructed; but we cannot conclude without awarding praise to the author for the scientific manner in which he has dealt with his subject, and for the style in which the work has been brought out.

[a] Lib. II. cap. xi. [b] Lib. VII. cap. xxix.

General Board of Health. Second Report on Quarantine. Yellow Fever. With Appendices. Presented to both Houses of Parliament by command of Her Majesty. London, 1852. 8vo. pp. 414.

When we were told about six months since that the London Board of Health was engaged in drawing up a Report on yellow fever, we confess we had our doubts as to how far its members were qualified for the task: still, by a careful analysis of all that has been written on the subject; by inquiries impartially addressed to those who have had more experience in tropical diseases than any of themselves; and, possibly, by the addition of new facts obtained from the coast of Brazil and the West Indies respecting the epidemics which recently prevailed in these countries, we thought it not improbable that something new, or at all events something more specific, as regards the disputed question of contagion, would have resulted from their labours. To those who have read the Report we need not say that we have been disappointed. In a work professing to have been prepared with a view to advance the public interests,—not by one person, but by the united labours of several,—we had a right to expect that the question would have been fairly examined, that preconceived notions and biassed views, whether they had reference to the well-being of the community or the interests of commerce, would have been set aside, and that truth alone would have been the goal towards which the Board would have directed its inquiries;—and in order that the members of the medical profession, and the world at large, might understand with what impartiality the inquiry had been conducted, that the arguments on both sides of the question on which the conclusions of the Board were to rest would also have been fairly and equally stated;—need we say that our expectations have not been fulfilled? A more, if we may use the phrase, "lob-sided" production was never before placed in the hands of the public. Following the method adopted by the distinguished Superintendent of Quarantine, the authors of the Report have carefully avoided, or placed in the shade, every fact or circumstance which could in any way militate against the views they wish to promulgate respecting the propagation of contagious diseases, and the utility of quarantine. They have wasted much time in endeavouring to point out the mote that is in Sir William Pym's eye, but have failed to detect the beam which is in their own. This is not what might have been expected from a Government Board.

In the following initiatory passage the authors of the Report endeavour to make us acquainted with certain views which they entertain respecting the questions of contagion and quarantine:—

"We apprehend the question of contagion has no necessary connexion with that of the importation of epidemic disease. The alleged importation of epidemic disease is a question of fact to be determined, like any other fact, by evidence. The evidence about to be adduced appears to us to be decisive against the allegation of importation, not because it is decisive against the hypothesis of contagion, but because it shows the utter insufficiency of the grounds laid to establish the act of importation. In like manner with respect to quarantine, if it were admitted that one true and invariable character of epidemic diseases is, that they are contagious, it would not follow that quarantine can prevent or even check their spread. Whether quarantine is capable of exercising any such influence or not, is a question to be determined, like other questions of fact, by observation and experience; and as we endeavoured to show in our former Report, the real practical question for the public and the Legislature is, not whether epidemic diseases are contagious, but whether quarantine can prevent their introduction and extension."

If we understand rightly the preceding paragraph, the Board of Health has a notion that the question of contagion, and the question of the importation of an epidemic disease by contagion, are different and distinct, having no necessary connexion with each other; but as to how any inquiry concerning the importation of a contagious malady may be conducted without reference to the malady itself, we are not informed. The evidence adduced against the "allegation" of importation, we are told, appears to be decisive, and so perhaps it may; but there is evidence of an opposite character, which the Board has thought proper not to adduce, that would place the allegation in a very different light. Were we to adopt the mode of collecting and arranging evidence followed by the Board, we might, with equal justice, attempt to prove that no case of yellow fever ever terminated fatally between the hours of twelve and one o'clock, or that the chameleon is still a blue animal, notwithstanding all that has been said to the contrary. Whether that kind of quarantine which may be enforced in civilized countries, in close proximity and intimately connected with one another by mercantile interests, is or is not capable of excluding an infectious disease, we admit, remains to be proved; but we cannot admit with the Reporters, that "the real practical question for the public and the Legislature is, not whether epidemic diseases are contagious, but whether quarantine can prevent

their introduction and extension." When the Government of this country seriously contemplates an inquiry into the question of quarantine, we should certainly recommend that it be preceded by an inquiry into the nature of those diseases which are known or supposed to be communicable.

We are informed that "the body of evidence given in the former Report on Cholera had not been impugned;" and "that quarantine, which proved not only useless but even mischievous, had no influence in checking the progress of that disease throughout Europe." In the present Report certain similarities, as well as certain differences, which distinguish yellow fever as an epidemic from cholera, are pointed out. The former, it is assumed, differs from cholera in two important circumstances:—"First, in being limited to a certain zone; and, secondly, in attacking those who have recently arrived [from a colder latitude?] within that zone or near its limits; while cholera spreads from continent to continent, ravaging the nations of all climates." The other distinction between cholera and yellow fever is, "that the former acknowledges no acclimatization, while the latter seldom attacks acclimated creoles." A resemblance between the two diseases is thus noticed:—"In cholera a peculiar poison enters the blood, in consequence of which it becomes disorganized, and a fatal discharge takes place of watery fluids derived from it" (the blood). The second point of resemblance between these two diseases is, the similar condition of the brain,—meaning, we presume, the functions of the brain:—"In cholera the intellect is clear, and the mind calm, up to the moment of death; and the functions of the brain in yellow fever are generally retained in the same perfect manner."

It is not, perhaps, a point of much importance, but our experience has led us to a different conclusion with respect to the state of the intellect in the last stages of the two maladies. In the last stage of yellow fever the intellect is generally much disturbed. Sometimes there is furious delirium, with convulsions; at other times low, muttering delirium, with great prostration; or, when dissolution is protracted for several days, the patient may lie in a somnolent or semi-comatose state, answering questions when roused calmly but stupidly; but it is seldom that patients die with the clearness of intellect peculiar to cholera. An extract, in which a few extraordinary cases are mentioned with respect to the state of the intellect, is given from Dr. Ferguson's work, but the meaning of that eminent physician has evidently been mistaken. The self-possession and courage which he alludes to is observed in the middle, not in the last stage of the disease. As to the existence of extreme

danger occasionally, when the patient (and even an inexperienced physician) may consider himself all but well, we entirely agree with Dr. Davy; but we can not agree with Dr. Blair, who tells us "that the patient dies with intelligence unclouded, and his muscular strength but little impaired."

With respect to the points of resemblance between cholera and yellow fever, we confess we are at a loss to perceive them, and had it not been that the comparison gave the Board an opportunity of referring to their former Report, we presume they might have detected a greater resemblance between that disease and small-pox. But there is even a greater resemblance still between yellow fever and plague. It will at once occur to those who are familiar with the history of that pestilence, that although it has generally been confined to a particular zone or region, it has, nevertheless, occasionally made its appearance in other parts of the globe; and although it spares not the Egyptian, it has generally been most destructive amongst the natives of colder regions.

We shall not attempt to give any analysis of the next twenty pages of the Report, because they contain but little more than a number of extracts from different well-known writers, interspersed with an occasional remark by the authors, but without any attempt at arrangement, or proof of the soundness of the views contained in these *disjecta membra*, in the collection of which the compilers appear to have considered themselves bound not only to perform the office of caterer, but of cook also; moreover in preparing the repast they seem to have been under an impression that quantity would make up for the quality of the viand. At pp. 15, 16, we have extracts from Drs. Gavin, Hennen, Smith, and Gardner, as to the simultaneous appearance of the disease at different places; then follow observations from Messrs. Wilson, Frazer, and Hort, to prove that epidemics are generally preceded by sporadic cases; and from other authors quotations are made to show that yellow fever during epidemics is frequently confined to one or two members of a family; that it is frequently limited to particular spots; that seclusion within the influence of the epidemic cause is not only ineffectual in preventing attacks, but that it is often injurious; that removing beyond the range of the exciting cause is often attended with advantages; that the progress of the disease may thus be suddenly checked; that patients in hospital for other diseases frequently escape yellow fever, although placed in wards along with persons suffering from that disease; that medical men, nurses, and other attendants, are not attacked from contact with yellow fever patients; that

occasional attacks in hospitals are to be attributed to the locality, or to poison previously received in an infected district; and that it is the unanimous opinion of medical men in the West Indies, that yellow fever is not contagious.

The latter assumption appears to be based on a remark casually made by Dr. Milroy, who observes that it was "the unanimous conviction of the medical men in Jamaica, and also of those in Havannah whom I met, that the disease is not communicated from the sick to the healthy." Of course the Doctor does not mention what number of medical men he met with in the West Indies, or whether he associated with all alike,—the contagionist as well as the non-contagionist. Of this, however, we feel assured, that if the Board of Health could induce Sir William Pym to visit the Brazils, on his return, he would tell us that he had met with no medical men who were not of *his* way of thinking. We do not question one word that Dr. Milroy has either uttered or written, but we question the propriety of the Board of Health using, in an official Report, hearsay upon hearsay evidence obtained in this loose manner.

Dr. Milroy appears to be in some doubt as to the reason why, in those countries where yellow fever is of frequent occurrence, medical men almost universally regard it as non-contagious. We shall explain to him why this is so. In the island of Jamaica, for instance, where isolated attacks occur occasionally, and now and then slight epidemics break out, there is always a large proportion of the white population protected by previous attacks, consequently it is difficult to trace the communication, or to understand why so many individuals escape; besides, if medical men in these regions, as is too often the case, direct their attention to those cases only which are occurring around them, they have not the means of judging whether the disease depends on a local physical cause, or on a contagious emanation from the sick, because it is impossible for them to distinguish the effects of the one from the effects of the other. Taking into consideration the doctrines which have been taught in the medical schools for the last twenty or thirty years, and the lasting nature of our early impressions, we think there is little difficulty in accounting for the opinions entertained by a considerable body of the practitioners in the West Indies. In this country there are few who question the communicability of scarlatina, yet how seldom in an infected district can we trace isolated cases to their source.

We have next, from pp. 41–45, several quotations, with interpolations by the Board, respecting the comparative mortality of yellow fever in different localities, and the influence of filth

as a "localizing" cause; but as these have little to do with the main question we shall pass them by, merely observing that the conclusions arrived at by the Board are of little value, inasmuch as they are based on a number of extracts, selected apparently with the view of supporting a foregone conclusion. We freely admit that which must be patent to the whole world, namely, that accumulations of filth, together with overcrowding, bad ventilation, foul drains, and the like, may be productive of ill health; but we deny that, of themselves, they are capable of producing yellow fever. If it were necessary, we could mention places almost without number, such, for instance, as Up-Park Camp and Stony-Hill Barracks in Jamaica, Sierra Leone and Goree on the coast of Africa, Bermuda in the North, and Ascension in the South, Atlantic, in which none of these conditions exist, yet yellow fever has prevailed as destructively in these places as in others abounding in filth and surrounded by marshes. Were it possible to place in juxtaposition the number and kind of diseases occurring in ill-drained and filthy localities, with the number and kind of diseases occurring in places of a different character, we would then be able to form an opinion as to how far the above conditions are instrumental in the production of epidemic diseases; but the method adopted by the Board of Health, in selecting but one kind of evidence, can only lead to error.

In the following passage the effects of different kinds of dampness is forcibly discovered:—

> "The influence of dampness in promoting the spread of cholera is universally admitted; the evidence is as complete that it is equally powerful in localizing yellow fever, particularly the dampness arising from the foul shores of the sea; the moist slime and mud on the banks of rivers, canals, pools, ditches, and the moats of garrisons; the mire and mud in the unpaved streets, lanes, alleys, and courts of towns and cities; and the oozings and percolations from sewers and privies, which are often the cause of the humidity of cellar dwellings."

It will be observed that the Board has here pointed out another remarkable difference with respect to cholera and yellow fever,—the one being spread, and the other localized, by dampness, particularly that kind of dampness which exists on the sea-shore, &c.; but as to where our learned authors obtained this piece of information they are silent; neither do they attempt to explain how the oozings and percolations from sewers and privies have an action so diametrically opposed to each other on the two maladies. Leaving us, as it

were, in the "mire and mud" of their discovery, they next adduce some remarks on the pernicious results arising from the mixture of salt and fresh water, in which they are supported by Drs. Davy and Browne. The latter informs us, through the Board, that this mixture is one of the conditions which is most favourable for the production of malaria, "probably from the destruction of organic life it occasions, and in fact from the elements which it furnishes for chemical re-action." Little does the unwary cockney think when he resorts to Gravesend for the purpose of breathing pure air and eating fresh shrimps, that, according to Dr. Browne and the Board of Health, he is all the while living in an atmosphere polluted by elements almost too horrible to mention.

The next ten pages, namely, from 52 to 60, are occupied by about double the number of quotations from the writings of Davy, Browne, Pringle, Gavin, and many others, all tending to show that diseases of different kinds have prevailed more generally and more severely in districts in close proximity to marshes than in others that are distant from them. We do not see the use of crowding evidence of this kind into a Report purporting to be on quarantine with relation to yellow fever. If the members of the Board cannot employ themselves otherwise than in the compilation of extracts from books which are in every medical man's library, or at all events in every medical library in the Kingdom, the sooner they resign their office the better for the country. Moreover, in the whole of the evidence thus ostentatiously brought forward, and furbished up with a running commentary by the Board, there is not a single observation that is not well known to every man into whose hands the Report is likely to fall. We, therefore, most energetically protest against a system of report-making which is discreditable to the scientific character of the country, and disgraceful to the Government which sanctions it. If "the accumulation of rank vegetation, with a urinous smell from a neglected hollow," gave rise to yellow fever in one locality, we should like to be informed whether hollows of a similar nature generally produce fever; and why it is that the "slime and mud" of the unpaved streets produce at one time a specific disease, and not at another,—an assumption, by the way, which is purely hypothetical, and as regards the presumed causes, we deny that they have any such influence.

At page 59 the authors of the Report acquaint their readers that Dr. Browne informs them, he has learned from the authors of a statistical Report that a fort at Nassau, which was nearly

surrounded with marshes, was extremely unhealthy in the last century; and, following up the subject in the same monotonous style which pervades the whole work, they quote the opinion of Dr. Milroy:—

"Several well-marked instances of the abatement or total cessation of fever, by draining and clearing the ground in the neighbourhood of dwellings, were mentioned to me in different parts of Jamaica. Mr. Bruce, one of the practitioners in the parish of Vere, quoted his own residence as a striking illustration of this fact."

Dr. Milroy we know to be a very talented physician and a clever writer, we therefore regret to see that no better use has been made of his labours than to cull from them such worthless passages (worthless as regards what ought to have been the aim and purpose of the Board) as the preceding. Of Mr. Bruce and *his pen*, in the parish of Vere, we know nothing; but this we do know, that had the Board of Health put themselves to the trouble, they might have found fifty illustrations of the same fact, all occurring within ten miles of Gwydyr House, or of any other house in which for the time they may succeed in localizing themselves. That Mr. Browne states that Dr. Smith said marshes produced fever, or that Professor Green observes that Sir Peter Jones remarked the same effect to follow the opening of a foul drain, is not the kind of evidence that is required at the present time to establish the identity of remitting and yellow fever; to prove that the latter neither has nor can acquire the property of self-propagation; or to show that it is connected with "localizing causes, definite, palpable, often restricted in their operation, and removable."

With respect to the influence of foul drains, swamps, marshes, stagnant pools, and the mixture of salt and fresh water, the Reporters exhibit at pages 61 and 62 a depth of reasoning which we had well nigh given up as unfathomable. It seems the Board considers medical authorities agree that it is not the conditions connected with unhealthy localities that are the causes of epidemics, but that it is rather the circumstances connected with these same localities which determine or cause the epidemic principle (when it is present) to act. In other words, the profession, not knowing very well or caring very much what the Board of Health means by "localizing conditions," does not recognise the malaria of towns and swamps as the essential exciting cause of epidemics such as yellow fever; but they acknowledge that epidemics are generally more destructive in localities abounding in malaria than they are in others that are free from them.

That yellow fever may be produced in the yellow fever climate by crowding Europeans in close, ill-drained, and filthy places, the Board considers "is placed by every-day occurrence in those regions beyond the possibility of a doubt." This is an assertion which we must call in question, because, notwithstanding a witty recipe given by Dr. Bone, "for the sure and rapid production of yellow fever," we do not believe that any credible evidence can be adduced in support of it; and because we know from experience that although fever may thus be produced, in ninety-nine cases out of a hundred, even within the tropics, it will not be yellow fever. The members of the Board of Health show that they are but little conversant with the subject they have undertaken to investigate, when they assume that yellow fever is of every-day occurrence either in the West Indies or on the coast of Africa. Notwithstanding the practice of crowding slaves in barracoons and in horribly filthy ships, in which there are always a number of susceptible Europeans, there is not, we will venture to affirm, a single instance known of yellow fever having broken out in either region in any one of these barracoons or vessels for the last ten, or it may be twenty, years. Even in the worst parts of Sierra Leone isolated cases of yellow fever are seldom met with. Black vomit occurs occasionally in other diseases besides yellow fever, both in this country and within the tropics; we therefore do not admit that every case presenting black vomit is unequivocally a case of yellow fever.

There are, we presume, few of our readers who are not aware that for at least the last fifty years, the contagious nature of yellow fever is a subject that has been warmly, if not fiercely, contested between two opposite factions in the medical profession; the one, successively led by Chisolm, Blane, Pym, and others, less violent, perhaps, but equally determined to stand by their cause, maintained that the disease was contagious; the other, led by Bancroft, Fellows, O'Halloran, Burnett, and Gillkrest, that it was not contagious. Throughout the whole of this extraordinary controversy there appears to have been a good deal of personal recrimination on both sides, and, it is to be regretted, not a little tergiversation with regard to the statement of facts. The truth has frequently been suppressed or turned awry, and the same circumstances bent and coloured to suit the views of either party. Hence, with respect to the matter of dispute, there is little dependence to be placed on any of the publications which speak of the disease previously to the year 1825, when Sir William Burnett's account of the importation of the disease into the island of Ascension turned

for a time the tide of battle against the non-contagionists. We are, therefore, of opinion that the Board of Health has acted most unwisely in opening up these old feuds. The facts connected with the Regalia, Donostiana, and Hankey, the fevers which ravaged different parts of Spain and the West Indies, are now so distorted and overlaid with contrary and conflicting statements, that it is utterly impossible to deduce from them any evidence respecting the contagious properties of the disease that would be deemed of the slightest value. But, in stating that the Board has acted unwisely in this matter, we have not stated all: we are also compelled to add, that it has acted most unfairly in quoting even from these questionable sources such passages only as are best calculated to bewilder the public mind, and to baffle any inquiry into their own statements. We question, indeed, whether the majority of the medical profession will not consider that they have likewise acted with great presumption in attempting, under any circumstances, to examine a subject for which, as a body, they are neither qualified by experience nor professional education. As it would, therefore, be but little better than a waste of time to enter into any examination of this part of the Report, we shall at once pass on to that in which the fever of the Bann is spoken of.

The fever made its appearance amongst the crew of this vessel at Sierra Leone in March, 1823, and continued to rage amongst them with great virulence until they arrived at the island of Ascension on the 25th of April, where the sick were landed and placed in tents at a distance from the garrison, which, however, did not prevent the fever from being communicated to the residents on the island. Sir William Burnett remarks, that "it was, in the first instance, merely the common endemic of the country, brought on by hard labour and exposure to the sun, not possessing, under these circumstances, any contagious properties, and continued to be so until after the middle of February; that it subsequently, by the state of the weather preventing ventilation, and from a great number of the sick being confined in a small place, became contagious; and that, though it was impossible to trace the fever in question directly from the Bann to any individual of the garrison of Ascension, yet there is just reason to believe that the disease was introduced into the island by that ship."

The plain, simple story of the Bann has been quoted by almost every writer on yellow fever for the last five and twenty years, and still it escaped mutilation until it came to be re-written for the Second Report on Quarantine, by the General Board of Health. In the 59th page of that remark-

able production it will be found condensed into seventeen lines, and these perhaps give us about as fair a specimen of the way in which the illustrious authors deal with facts, as is to be found in the whole book. We here present them to our readers as a literary curiosity:—

"In the year 1823 an opinion was advanced by Sir Gilbert Blane, whose eminence and official position gave to any statement made by him the weight of authority, that the 'Bann' sloop of war, sailing from Sierra Leone, introduced yellow fever into the island of Ascension. This case was examined by Sir William Burnett, who published an official report upon it, from which it appears that Sir Gilbert Blane was in error as to the state of the health of the crew of the 'Bann' when she left Sierra Leone; that disease of precisely the same character as that alleged to have been imported by the 'Bann' had existed and been prevalent in the island at former periods; that after the most careful inquiry it was found impossible to trace the fever in question directly from the 'Bann' to any individual of the garrison of Ascension; and that the first person attacked was certainly not known to have been in the ship, or in contact with the sick."

It would hardly be supposed, from the preceding statement of the Board of Health, that yellow fever had been imported into the island of Ascension, and most certainly it could not be supposed that Sir William Burnett, also "a man of eminence and high official position," and a firm believer in the non-contagious nature of the disease, could have admitted that it was imported; nevertheless we have just seen that Sir William had the candour and courage to state honestly that which he could not honestly deny.

From the fever of the Bann the Board of Health, following its devious course, next turns to the case of the Eclair; but as we have in our eighth volume entered fully into this question, we shall proceed at once to consider the introduction of the fever into Gibraltar in 1828, the more especially as it comes next in chronological order. Until the publication of the present Report we had deemed the history of the eruption or introduction of this epidemic was, like those which preceded the Bann, utterly lost to the profession, in consequence of the irreconcilable and contrary opinions held as to its source; but we have now to thank the authors of the Report and Mr. Howel for clearing away much of the obscurity in which it was formerly enveloped.

We shall make no apology for using the following extract as a kind of preface to our remarks:—"We beg," say the illuminati of Gwydyr House, "to call special attention to this

case, because it has been more rigorously examined than any other, and because on that account it exhibits a better specimen than can usually be obtained of the manner in which the evidence for these cases is commonly got up."

It would appear the ship Dygden left the Havannah about the 12th of May, 1828, with a clean bill of health, which did not state that there was no yellow fever at the Havannah, but that the city was free from plague or contagious epidemic disease. Two men out of a crew of sixteen died at sea; one on the 27th of May, and the other on the 1st of June. Both the bodies and the clothes of the men were immediately thrown overboard. The ship arrived at Gibraltar on the 28th of June, and was placed in quarantine. But in consequence of some rumours that were circulated against her, she was kept under surveillance by the health officer, Dr. Hennen. "It appears by the evidence of the Deputy Pratique Master that he does not recollect, during the eighteen years he has held his office, one single instance of health guards being put on board, or of fumigation being applied to any ship from the West Indies before the Dygden." So much for the ship.

On the 11th or 12th of August the disease made its appearance in an obscure yard in Gibraltar, and soon afterwards spread in various directions over the town. Consequently, but not until the following April, a Board was instituted to inquire into the circumstances connected with the eruption of the disease:—

"Evidence was first adduced to the Board on the 8th of April, 1829, when M. Villalunga stated that she lived in the yard of Fenic's house; that the first person attacked was the boy Fenic; he was taken ill on the 11th or 12th, and died on the 17th. His sister, a little girl, was seized on the same day (the 17th) and died on the 20th. Then another boy, named Caffiero, was attacked about the 19th or the 20th; he recovered. The same woman stated, that 'she heard the boy say, that his sister and father had been on board a ship in the bay on Sunday (the 10th), the day before the boy took ill; that his father had sold some tobacco,—about an arroba of cigars,—as a sample."—p. 247. "A low practitioner of medicine, named Cortes, stated to the same Board, that Salvador and Felix Fenic were, as previously stated, taken ill, and died, and further, that he was informed by Dr. Lopez, who attended the children, that they died of yellow fever."—p. 248. "The child Caffiero, who recovered, was examined on the 10th of April; he stated that he saw Salvador and Catalana Fenic every day when they were sick in bed, and again on the 18th; he stated that he went on board the ship with the Fenics, but did not recollect the day; they remained about an hour. Fenic, the father, who rowed the boat, ate and drank on

board, and brought a bundle of clothes on shore. He further said that his mother was a washerwoman; that, on Fenic's wife refusing to wash the clothes, they were offered to his mother, who also refused them. When we landed, Fenic gave me a pistoreen, and told me not to say anything to anybody about our having been on board."—p. 249. "Josefa Salinas deposed: 'I recollect that Fenic's daughter, who is dead, came to my house one day without my asking her, and told me that she had been on shipboard with her father and brother,—this was a little before the epidemic; she said she had been sea-sick; she said her father had been on board to sell cigars; that he sold about an arroba; she gave me no particulars, as I asked her no questions.'"—p. 251. "Another person, Gonzales, said, 'that Catalina Barne told me that the cause of the death of Fenic's children had been the foul clothes brought from the ship, and not the eating green figs, as was reported."

There is other circumstantial evidence, which it is unnecessary to state, as the preceding, we imagine, will satisfy most people as to the communication with the ship. Still the authors of the Report and Mr. Howel consider that the evidence breaks down, inasmuch as the statements made by Villalunga and the boy Caffiero do not exactly agree in some minor points, as to dates, at the different times they appeared before the Board. We can only say, if we may be permitted to give an opinion in the face of such authority, that men are hanged every year in this country on evidence that is far less satisfactory.

With respect to the evidence of the boy Caffiero, Mr. Howel declares:

"Evidence such as this, and given as I saw it given, bears on its face every character of falsehood; and disbelieving as I do this boy's whole story, and at the same time considering his extreme youth, the testimony given by him has upon my mind the further operation of tainting with more than suspicion all the other evidence proceeding from the same class of witnesses, which consisted chiefly of hearsay in conversations with persons who had since died; because it would seem that this child must have been an instrument in the hands of some one of maturer age."

We fear that Mr. Howel has not seen much of the better side of human nature; but let us examine what kind of evidence *he* brings forward to disprove the communication with the ship.

"The only remaining witness concerning the introduction of the epidemic into Felix Fenic's family is the widow of Fenic himself. She was examined on the 25th of April. She was, at my desire, particularly reminded that the duty which she owed to society re-

quired her to disclose everything that she knew, and from the ingenuous manner in which her evidence was given, I am led to believe that she spoke the truth.

"She declared that she did not know the cause of her children's illness: 'they were attended by Dr. Lopez, who is dead, and who said they had a tabardillo and indigestion, *caused by eating green figs.* He did not say what was the the cause of the tabardillo. My husband was a cigar-maker; but he did not go on board ship either to buy tobacco or to sell cigars. Neither my husband nor my children went into the bay at any time during last summer or autumn. I know this, because if they had gone, they would have told me, and they did not tell me.'"

The Board and Mr. Howel, throughout the whole of their comments on the case of the Dygden, constantly allude to unfair means having been used to procure proof confirmatory of the communication with the ship; but we cannot see why, if they were not guilty of dishonesty themselves, they should suspect others of acting dishonestly. But we now leave it to our readers to judge whether the concurrent testimony of a number of witnesses (all equally respectable with the widow),—one of whom, be it remarked, was a child of eleven years of age, and therefore not likely to be selected to give false evidence, although, perhaps, when cross-examined by so sharp a practitioner as Mr. Howel, not able to make the same statement at different times in precisely the same words,—whether, we say, it is more probable the six witnesses were suborned, and that their guilt escaped detection, or that Mr. Howel's one witness—"*mindful of the duty she owed to society*"—was prevailed on to make a false statement for some purpose best known to herself. At p. 223 of the Appendix it is rather quaintly mentioned that this witness was ready, if required, to corroborate her statements on oath.

Whether the deaths which occurred on the passage between the Havannah and Gibraltar were or were not from yellow fever, can now be only regarded as a matter of conjecture; but we know of no disease which at that time prevailed on board ships in these regions, which was likely to prove so rapidly fatal, and to induce the crew to throw the clothes of the dead overboard. The time between the last death and the eruption of the disease in Fenic's yard, namely, forty days, is far beyond any ascertained period of incubation: but is it beyond the time that a specific contagion will exist in the form of fomites? Then, notwithstanding the assertion of the master, it is not improbable that some of the crew may have suffered from mild attacks subsequently to the 1st of June. Reports

were rife that they had arrived in a sickly state, and that several men had died even while the vessel lay in Gibraltar Bay.

These circumstances are at all events suspicious, and when taken in connexion with the appearance of the first cases of the epidemic in the three or four individuals who visited the ship, we must say that the evidence in favour of importation, however inexplicable, preponderates vastly over the conclusions arrived at by the Board of Health, namely, that the disease resulted from the foul state of the drains throughout the city.

But there are other grounds on which the Board, by adopting Mr. Howel's views, endeavour to dispute the possibility of importation. That gentleman, after condemning in most unqualified terms all the evidence opposed to that extracted from the widow Fenic, observes—

"Here I leave the journals of washerwomen and the tattle of their gossips, remarking this fatal objection to each washing-tub anecdote, however circumstantial,—that *not one of them goes back so far as to precede*, and therefore to account for, *the alleged first case of the epidemic*, namely, that of Salvador Fenic, who, as we are told, fell ill *on the 11th of August*, and upon whose single case, therefore, the proof of importation rests. And if the attempt to connect the illness of Salvador Fenic with a foreign source be, as I hold it to be, a complete failure, how is the illness of the boy Caffiero to be accounted for?"

We do not see the difficulty of connecting the illness of the boy Fenic with some cause existing in the Dygden. He visited the ship on the 10th, and fell ill on the 11th or 12th of August, thus affording sufficient time for the evolution of any specific contagion imbibed in the vessel. And as to the boy Caffiero, it is not possible to form an opinion as to whether he contracted the disease on board or from his playmates on shore.

It has been shown that the Board of Health did not venture to impugn the testimony of Sir William Burnett with respect to the importation of yellow fever into Ascension: neither have they ventured to say one word concerning the importation of the disease into the Gambia by the Curlew, in 1837; into Goree, by a merchant vessel in the same year, as detailed by Staff-Surgeon Ferguson; or into Ascension for the second time in 1838, as detailed by Dr. Bryson. Whether these cases escaped the notice of the Board, or whether they were thrown aside as troublesome and unsuitable for their purpose, we need not offer an opinion. But the case of the Eclair could not be so easily disposed of. It was of recent occur-

rence, and had excited a good deal of attention; it had been thoroughly investigated by Dr. M'William, and subsequently Dr. King went over the same ground. The Report made by the former had been strictly examined and reviewed by us and most other medical journals in this country, in France, and in America. The facts carefully collected by him were admitted to be sound and incontrovertible, his deductions clear and logical. Dr. King's Report passed, but to a much less extent, through a similar ordeal; his facts as regards the main features of the case were substantially the same, but his conclusions, based on an improbable assumption, were condemned as erroneous and illogical. But our space does not permit us, anxious as we might be to do so, to again open up this question, and to expose, as we easily could, the futility and falsity of the arguments put forward by the Board of Health in their present Report.

The case of the Eclair and the introduction of the fever into Boa Vista, occupies about twenty pages of the Report, but notwithstanding the pains which have been taken to break the chain of evidence first clearly traced out by Dr. M'William, we must say that not one link of it has been disturbed; and we can only repeat what was stated by us on a former occasion[a], "that the conclusions flow as legitimately from the premises as moral reasoning will admit of."

We have now noticed the principal subjects brought under consideration in this most singular Report. We have shown that its authors broke ground by a kind of incomprehensible quibble respecting the question of quarantine and contagion; that they made an attempt to draw a comparison where hardly any points of resemblance exist, and that by evidence purposely arranged, and therefore fallacious, they have endeavoured to show that within the tropics yellow fever is greatly influenced by certain local causes. We have pointed out the peculiar course taken by the authors of the Report, in quoting from the controversial writings of the older authors such passages only as were calculated to mislead the public and establish their own views; and that they have—shall we say purposely—abstained (with one notable exception) from noticing any of the more recent publications in which the evidence respecting the propagation of the disease is less hampered with conflicting opinions, or with local circumstances, tending to obscure the facts necessary to the formation of a just opinion; that they evaded noticing the fever of the Bann by a discreditable sub-

[a] New Series, vol. viii. p. 192.

terfuge; and that they failed to make their case good with respect to the Dygden. But they failed more signally still in their attempt to disprove the importation of yellow fever into Boa Vista by the Eclair. We might also have shown that, by confounding yellow fever with the common endemic of warm climates, they displayed a want of knowledge respecting the fevers which attack Europeans within the tropics; that while they made the most unscrupulous use of other men's labours, they have not contributed even one additional fact of their own; that they have presented a Report on yellow fever to both Houses of Parliament based partly on epidemics which existed in the last century, while they appear to have thought it unnecessary to enter into any inquiry respecting those that have occurred within the four or five last years! Truly, then, it may be asked, whence originated the demand for this Report? And why was it intrusted to a Board so notoriously disqualified for the undertaking? Disqualified, not only from a want of practical knowledge respecting tropical diseases, but from a want of practical knowledge respecting the right use and application of the common law of evidence. We trust the British Parliament will in future guard against its august name being prostituted for the purpose of stamping with the colour of authority any medical publication emanating from a non-medical body, until it has been examined and approved of by men of learning and wisdom in the profession. We cannot, in fine, close this notice without calling on the various medical corporations throughout the Kingdom to use their best endeavours to put an end to this spurious system of fabricating Medical Reports, a system not only insulting to themselves, but discreditable to the medical literature of the country.

Lectures on the Principles and Practice of Midwifery. By EDWARD WILLIAM MURPHY, A. M., M. D., Professor of Midwifery, University College. London: Taylor, Walton, and Maberly. 1852. 8vo. pp. 616.

NEARLY five years have elapsed since Dr. Murphy published the First Part of these Lectures, which treated of natural and difficult parturition, and was reviewed in the Second Number of our New Series. The subject of the Lectures contained in the Second Part of the volume includes "preternatural and complex labours, and lactation," and to their consideration we shall devote as much of our space as can reasonably be appropriated to a second notice of the same work. It may, perhaps, be asked,

what need was there for the publication of a book of this kind, when the admirable treatises of Ramsbotham, Lee, Rigby, Collins, and Churchill are in our hands? This question it is not for us to answer; it solely concerns the author and the publishers. So, leaving to them its solution, we shall lay before our readers such portions of Dr. Murphy's Lectures as are deserving of comment, or possess originality, by which course it is hoped a fair estimate may be formed of the merits of the work.

British accoucheurs of the present day may conveniently be arranged into two divisions. One of these is represented by Dr. Robert Lee and Dr. Collins, with the Dublin School generally. The characteristics of this class are, great reserve in having recourse to interference with parturition, except upon the clearest evidence of its necessity; strong reliance on the resources of nature; and extreme caution in abandoning established rules of practice. Hence, then, their obstetrical principles may, in a word, be described as possessing a conservative tendency; whilst those of the other class exhibit a greater predilection for novelty and innovation, and less regard in differing from long-recognised maxims and doctrines. They may, therefore, with all truth and courtesy be styled *reformers:* so that in obstetrics as in politics we have our protectionists and our radicals. To Dr. Simpson must, *par excellence*, be assigned the foremost place in the ranks of the latter. Now the excellent author of these Lectures plainly shows himself to be a disciple of the same school, although brought up and indoctrinated in the former; and this fact inclines us to lend a more favourable ear to the reasoning and arguments that he adduces in defence of the new views and modes of practice which modern research and the ingenious application of statistics have discovered.

The portion of the volume we are about to notice commences with an account of preternatural labours, which are discussed in Lectures XIV. and XV. The matter contained in them is most excellent, but the rules of treatment are not quite so minute as might be desired, considering that the author's observations are addressed to pupils. There exists much diversity of opinion as to which hand is the best to use when turning the child. Most practitioners employ the right hand on all occasions, and a few prefer the left as being smaller, and possessing more acuteness of touch. Mr. Roberton's rule is to turn with "that hand which corresponds to the side of the body on which the patient is lying;" while Dr. Murphy tells us that "in general it will be found more easy to turn with the same hand as that presenting in the vagina." This, for-

tunately, is not a point of vital importance, though one of some consequence in difficult cases, where the most trivial manœuvre that may facilitate the operation should be put in practice. In theory the only true rule applicable to all cases would be to use the right arm when the legs of the fœtus occupy the anterior part of the uterus, and the left when they are next its posterior wall; the woman lying on her left side in both cases. The author agrees with Drs. Radford, Hardy, and M'Clintock in the propriety of bringing down only one foot, a practice which was frequently adopted by Chapman. Dr. Simpson insists that it is of the most essential importance to turn with the foot or knee of the limb on *the opposite side of the body* to that which is presenting. If this one can be distinguished and seized, it is certainly better to follow Dr. Simpson's advice, as with this limb the rotation or evolution of the child will be more certainly and more easily effected.

The subject of hemorrhage receives very full consideration, and, in connexion with it, the vascular relations of the uterus and placenta are minutely examined. The causes generally stated to produce detachment of the placenta in accidental hemorrhage are, shocks, violent exertions, mental emotion, spasmodic contractions of the uterus, &c., but to Dr. Murphy not one of these appears sufficient, *per se*, to separate the placenta from the uterus. His opinion of the way in which they act is by exciting the circulation, consequent upon which some of the delicate utero-placental vessels are torn, and blood is effused in greater or less quantity. It is more than probable, however, that in certain diseased states of the after-birth, its partial separation from the uterine wall may be the immediate and direct consequence of spasm, shocks, or external injury of any kind. That fatty degeneration of the organ can have the effect of disposing it to become detached under the operation of comparatively trivial causes may be looked upon as proved by the researches of Dr. Robert Barnes[a].

Nægèle has said of unavoidable hemorrhage, "there is no error in nature compared to this, for the very action which she uses to bring the child into the world is that by which she destroys both it and its mother" (Rigby). Dr. Murphy, who is a great admirer of nature, will not permit us to consider this proposition as true in its entire extent, and he labours to show that "nature is consistent with herself, and even here establishes the possibility of life being preserved by her own efforts."

[a] On Fatty Degeneration of the Placenta, Medico-Chirurgical Transactions, vol. xxxiv.

That such does occasionally happen in these desperate cases is not to be denied; the only question is, how, or in what manner, is it accomplished? Dr. Murphy endeavours to prove that "dilatation of the uterus controls unavoidable hemorrhage," and forthwith he proceeds to demostrate the manner, premising, that if this proposition be not true, "Nature has committed a capital error; she sins against her great law to do nothing in vain, and gives life only to destroy it." We admit that the hemorrhage has two sources, viz., the disjoined surfaces of the placenta and uterus, but the discharge from the former is comparatively so unimportant (from the readiness with which coagulation takes place in the cavernous structure), that the main source of loss is the exposed venous orifices in the uterine surface. We are to consider, then, how the dilatation of the os uteri restrains the discharge from this source.

"The dilatation of the os uteri is in fact the contraction of the cervix; the womb could not open unless the tissue of the cervix contracted upon itself. I do not assume that this contraction of the cervix is a muscular effort, as some suppose; it is sufficient for this explanation to admit that the cervix possesses contractility of tissue. Now this contraction of the cervix has precisely the same effect upon the veins here as the muscular contraction of the fundus has upon them in other forms of hemorrhage: the sinuses (as they are called) are more or less closed, the veins are compressed, the temporary valves set up, and thus the regurgitation of blood from this source prevented. As the dilatation of the uterus advances, the whole of the exposed portion of the cervix and placenta is directly compressed by the head of the child, an additional aid in preventing the escape of blood."

Now comes the practical application of these physiological doctrines:—

"The natural means, therefore, of checking unavoidable hemorrhage is the complete separation of the placenta from its attachment to the cervix of the uterus; because by this means all the uterine arteries are broken off from the placenta, and the veins are closed by the dilatation of the uterus, which is necessary to effect the separation."

It is not to be lost sight of that by this process the fœtus is necessarily destroyed. In it, therefore, nature has committed a "capital error," having "given life only to destroy it;" so that the author's own argument can thus be turned against himself. How, then, are we to explain nature's intention in this special case, and make it consistent with her usual wisdom and foresight in providing for the preservation of the species? To this ques-

tion there is an easy solution. The mother's only security lies in the complete expulsion of the ovum; the child's in its delivery before, or immediately after, the placenta. Hence it may fairly be inferred, that nature's mode of treating these cases is by the rapid dilatation of the os uteri and speedy completion of the labour, on which alone depends the preservation of the two lives. In accordance with this we find that of 101 cases cited by Dr. Simpson, where the length of the interval was noted, the child was expelled with, or immediately after, the placenta, in *thirty* instances; and in *twenty-four* instances this event took place within ten minutes after the placenta; and out of these fifty-two cases *twenty-two* of the children were living, being the only live births in the entire number recorded in Dr. Simpson's table. If, then, this argument drawn from nature has any weight, it is surely on the side of turning the child, and delivering the woman as expeditiously as possible. Admitting, however, Dr. Murphy's reasoning to be correct, and that under active pains the hemorrhage ceases as soon as the placenta is detached from the uterus, does this warrant us in concluding that the *artificial* separation of the placenta when the os uteri is undilated, or when no labour pains at all are present, will be followed by the prompt suppression of the discharge?

In some cases of unavoidable hemorrhage, with a contracted state of the os uteri, he thinks well of evacuating the liquor amnii, as suggested by Dr. Radford, but in a very different manner. Dr. Radford's plan was to puncture the membranes through the placenta with a trocar. It must be imagined that this operation exposed the presenting part of the child to the danger of a punctured wound. Dr. Murphy also objects to it, but solely on the ground that some of the large vessels ramifying on the fœtal surface of the placenta might be injured. To avoid this he recommends a large gum elastic catheter to be made, and provided with a pointed stilette passing quite through it, and capable of being retracted within the extremity. This instrument, having the point sheathed, is to be passed within the os uteri, and between the placenta and uterus; and when it is quite above the placenta, the point is to be pushed forwards through the membranes: the stilette being then withdrawn, the waters are discharged through the catheter. We mention this instrument solely to express our strong condemnation of it, and our great surprise that Dr. Murphy could, in unqualified terms, recommend *to students* the use of such a dangerous contrivance. In cases of extreme exhaustion, when the shock of turning might not be borne, and in cases of rigidity of the os uteri refusing the passage of the hand, the artifical separation of the

placenta is recommended should the hemorrhage persist after the employment of the ordinary measures. These are the two cases in which this mode of treatment is advised.

We pass with much pleasure from this chapter to that on puerperal convulsions, which is one of the best in the book, evincing sound judgment and matured reflection. Discarding the ordinary classification into epileptic, apoplectic, and hysterical convulsions, he divides them into three orders, viz.:—1st, Sthenic, or hyperemic convulsions; 2nd, Asthenic or anemic convulsions; and, 3rd, Hysterical convulsions. The first of these is by far the most common, as it comprehends nearly all the cases usually included under the heads of epileptic and apoplectic convulsions. Asthenic convulsion is the final symptom in extreme hemorrhage; in this instance clearly arising from a total deficiency of blood to support the vital functions. Cases are also occasionally met with in which the same effect seems to be induced by an insufficiency of nutrient blood in the system. In regarding apoplexy as a consequence and not a cause of convulsions, we think Dr. Murphy has judged rightly. The results of our own experience entirely conform to this view. Cases of convulsions with apoplectic effusion have, no doubt, come under our observation,—in two instances the clot completely filling the lateral ventricles,—but in all these cases this particular lesion was most unquestionably occasioned by the severity of the fits, and the neglect of sufficiently prompt depletory measures. Upon the nature and causes of eclampsia, Dr. Murphy enters at considerable length, and contrasts the peculiarities of this disease and epilepsy. In some of the latter points his views closely coincide with those of Dr. Tyler Smith. As the result of a protracted discussion of these and allied questions, he thus sums up:—

"1st. Puerperal convulsions should not be confounded with epilepsy, nor with apoplexy. They agree with the epileptic attack in their physiological, but not in their pathological, characters. Apoplexy is an effect of the paroxysms, which may or may not follow from them.

"2nd. The predisposing causes of puerperal convulsions are either an excess of blood (hyperemia), a deficiency of blood (anemia), or impure blood (toxemia).

"3rd. The proximate causes of convulsions are chiefly *excentric*, being the morbid irritation of the afferent nerves supplying the different vital organs. Impure blood, as in albuminuria, is *a centric cause.*

"4th. *Morbid irritation of the uterus* is the most common proximate cause of puerperal convulsions, the result either of hyperemia or anemia. Hence the division into sthenic or hyperemic convul-

sions, and asthenic or anemic convulsions. Under the latter head we include not merely loss of blood, but poverty of blood, because the effect seems to be similar, only differing in degree.

"5th. *Morbid irritation of other organs* also causes puerperal convulsions, because, during pregnancy, and at the time of labour, the nervous system is more excitable than at any other period; and hence any organ may easily be rendered morbidly irritable. Puerperal convulsions so caused are much more fatal than the former, because the nervous centres are exposed to a twofold source of irritation,—the organ primarily affected, and the uterus that is secondarily excited.

"6th. In the whole of these phenomena we must perceive a beautiful illustration of the reflex nervous function; the peripheral nerves that supply the affected organ rapidly communicating the irritation to the spinal system, which, as an excito-motor centre, radiates the irritation over the whole of the voluntary muscles, and the muscles of respiration, in violent convulsive paroxysms. Even the involuntary muscles, as the uterus and heart, do not escape, but give every evidence of greatly increased muscular contractions."

Respecting the treatment of convulsions, very excellent rules are laid down for our guidance. Though not by any means timid in resorting to artificial assistance, still in these cases Dr. Murphy is rather averse to interference with the labour process, merely on account of the complication. To this opinion he inclines from the careful statistical examination of a large number of cases recorded by various authors. Against turning the child he especially cautions us, unless it be absolutely required on account of the presentation. Should the head descend within reach of the forceps, he says, we may apply them if the paroxysms are continuing; but if they have subsided, it is better not to do so, lest we might renew them. He further adds:—"If the head be impacted, or if it be difficult to apply the forceps, and the child's death be ascertained (and this usually soon takes place), you may remove it by the crotchet; but I would caution you strongly against turning the child." This is prudent and judicious advice, and as such we rejoice to see it brought prominently before the attention of students. It strongly contrasts, however, with what we shall now mention. In the chapter on rupture of the uterus, when alluding to separation of the cervix uteri, he takes occasion to reiterate his approval of a practice recommended in the former volume of his Lectures, and which we felt it our duty to censure when reviewing it. He tells his pupils that when they meet with embarrassing cases, "where the labour is prolonged, the os uteri undila-

table, and the child living, I feel justified in recommending you to incise the cervix for its relief." We feel perfectly convinced that under proper management, or even with no management at all, such cases would not occur once in ten thousand labours. When the fœtus has escaped through the rent into the peritoneal cavity, the only operation that appears to Dr. Murphy to give a reasonable chance of success is gastrotomy. This inferential advice he founds on an analysis of 118 cases of rupture quoted by Dr. Trask, in which some were delivered by turning and some by gastrotomy, and the rest were left to nature. If this be the fact, it is diametrically contrary to what would, *à priori*, have been supposed. Upon the after-treatment required by this accident nothing almost is said,—a serious and unaccountable omission.

We must pass over the chapters on inversio uteri, prolapse of the funis, and plural births, though they contain many observations on which we would wish to comment. The consideration of twin births is dispatched in two and a half pages, whilst forty are appropriated to ætherization. In connexion with this subject, a great deal of space is occupied in the discussion of matters that might, we think, have been just as well omitted, considering this is not a treatise upon materia medica or chemistry.

We have now arrived at the Third Part of these Lectures, but we can afford only a very brief allusion to one of the subjects therein treated, viz., puerperal fever. This term is employed by Dr. Murphy in a very limited sense, conformably with his views of the nature of this disease. He regards puerperal fever as an essential fever, not dependent on any local disease, but on the presence of a special morbid poison in the system, which poison exerts its fatal influence over all the phenomena that are observed in the disease. On this assumption, he refers puerperal fever to the class of zymotic diseases; "and we shall find in it," he writes, "as complete an illustration of the laws of morbid poisons as in any other of these disorders." He holds, therefore, that puerperal fever is totally distinct from metritis, peritonitis, or phlebitis. This last presents the greatest difficulty in drawing the line of demarcation, and for an obvious reason. In both diseases the blood becomes poisoned,—by pus in phlebitis, by a special morbid poison in puerperal fever. The essential difference between true puerperal fever and the many diseases which very much resemble it "exists in the presence of a new element in puerperal fever not met with in ordinary inflammation." The same idea seems

to have occurred to the mind of another physician, whose laborious researches into the history of this plague we have all profited by. In 1849 Dr. Churchill thus expresses himself:

"I have latterly seen reason to doubt the truth of the view I formerly took, which was in accordance with that of Dr. Lee; and though I would wish to express myself cautiously and guardedly, I must honestly avow, that whilst I fully admit the existence of local disease, I do think that epidemic puerperal fever is something more than that, although I may not be able to define exactly what it is." . . . "But then comes the question, what more is it than a local disease? We may call it puerperal fever *plus* the local disease, but this answer is neither very precise nor very positive."

Again he remarks:—

"It appears possible, at least, that the general element which constitutes the difference between epidemic puerperal fever and simple inflammation of the uterus and peritoneum may be some deterioration of the blood, depending either upon atmospheric malaria from without, or absorption of some noxious matter within the body"[a].

Dr. Ferguson, more strongly than any one else, perhaps, advanced the doctrine, that the phenomena of puerperal fever depend on a single cause,—vitiation of the blood. In contrasting the symptoms of puerperal fever and peritonitis, Dr. Murphy states:

"If you ask me for a diagnostic symptom, I should direct your attention to the influence of inflammation on the intestines. Pure inflammation of the peritoneum at once suspends their action and produces constipation. That inflammation which belongs to puerperal fever has no such effect; on the contrary, diarrhœa is often one of its earliest symptoms."

These quotations will, it is hoped, convey some idea of the salient features in Dr. Murphy's description of puerperal fever. Though possessing little of absolute originality, his theory, nevertheless, is well sustained, and applies with much exactness in explaining the phenomena and pathology of this disease. Beyond this it can hardly be said to serve any useful purpose. His views of puerperal fever lead him to describe the various inflammatory affections usually regarded as so many of its causes, under the general heading, "Post-partum Inflammations." Hence he treats separately of *inflammation of the mucous membrane of the vagina and uterus*, sthenic and asthenic;—

[a] Historical Sketch of the Epidemics of Puerperal Fever, contained in the volume of Essays on Diseases of Women, edited by Dr. Churchill, for the Sydenham Society.

inflammation of the fibrous structure of the uterus; peritonitis; inflammation of the sub-peritoneal tissue; uterine phlebitis, &c. These distinctions appear too artificial and refined; in active practice they never could be followed out, and would therefore only tend to confuse and perplex the student. This subject of puerperal fever is investigated by Dr. Murphy with much commendable care and minuteness; the ability and address with which he handles so capacious a question deserve the highest praise.

Our opinion of this work, as a whole, may readily be anticipated from what has preceded. In all that is anatomical, physiological, descriptive, and theoretic, relating to obstetrics, it cannot be surpassed, and gives the most recent information in a well-arranged manner and agreeable style. Placed in a purely practical point of view, however, we cannot bestow on the work such unqualified commendation. There are some points of practice respecting which we think the precepts laid down are unsound, and especially calculated to lead to mischievous results when forming part of the system of instruction given to pupils, or for the guidance of inexperienced accoucheurs. Improvement, it is true, cannot be effected without change, but in midwifery any change or innovation not tending to simplicity of practice must prove a positive evil, if not a positive good. An accurate observer and able writer of the present day lays it down as a principle, "that the natural progress of labour is less interfered with in proportion as obstetric science advances" (Roberton). Now if we are to receive this as a just criterion, it is to be feared that the "third estate of medicine" is liable to retrograde in the hands of some of its modern improvers. After making due allowance for the defects we have adverted to, Dr. Murphy's Lectures deserve our warmest praise. Their perusal has afforded us very considerable gratification, slightly tinged with the regret that their author should have renounced some of the practical principles of our School, in which he was educated, and of which he promises to be so distinguished an *alumnus*.

Om Byggnaden af Luftrörshufvudet hos den fullväxta Menniskan. Af Carl Fr. Naumann. Lund, tryckt uti Berlingska Boktryckeriet. 1851. Royal 8vo. pp. 64, with 6 Plates.

On the Structure of the Larynx in the Adult. By Charles Fr. Naumann.

In the first division of his work Dr. Naumann presents a carefully drawn up review of the several descriptions of the ana-

tomy of the larynx given by the principal writers on the subject, from the days of Hippocrates to the present time, chiefly with the object of pointing out the progressive development of their acquaintance with it. In the second part he describes with great minuteness and accuracy the cartilages, ligaments, muscles, vessels, nerves, and expletive (or areolar, adipose, and elastic) tissues, entering into the constitution of this organ, as well as its cavity, with the structures found in it, viz., the mucous membrane and its glands. The author does not enter upon the physiology of the larynx; and observes, that as its anatomy has for about two thousand years occupied the especial attention of investigators, it is not to be expected that he can now bring forward much that is new: he considers, however, that he has described several things more clearly than had hitherto been done, and that he has succeeded in deciding some disputed points. The following description of the final distribution of the nerves in the mucous membrane of the larynx will probably be interesting when compared with Wagner's views as to that in the fingers, given in our last volume[a]:

"The more minute distribution of the nerves can only be observed with the assistance of the microscope. This distribution was first described in reference to the transverse muscles by Prevost and Dumas, and subsequently by many anatomists. All agree in stating that the nervous branches which enter a muscle divide at first irregularly in all directions, but that the smaller subdivisions thus given off quickly assume a direction corresponding to that of the bundles of muscular fibres. By degrees minute fasciculi of nervous filaments are given off from these smaller subdivisions, crossing the muscular fibres transversely or obliquely, and forming repeated anastomoses, the 'plexus terminales.' Lastly, nervous filaments separate and form terminal loops, i. e. they unite in pairs by their extremities in a rather large-sized arch, which sometimes embraces several bundles of muscular fibres. The final distribution of the nerves in mucous membrane has, on the contrary, at least in the human subject, as yet been but little investigated. A tolerable number of nervous branches enter the mucous membrane of the larynx, and immediately divide in all directions into smaller filaments, which generally run a long, straight, or slightly serpentine course between, under, and above similar fasciculi from other nervous branches,—thus giving the appearance of a rather close network: at the same time we observe here and there actual anastomoses between pairs of such minute branches. Lastly, these united branches again become resolved into their primitive filaments, generally separating into equal pairs, which run a shorter or longer

[a] Page 492.

course, frequently form new anastomoses with each other, and finally produce terminal loops of various forms and sizes."

While the descriptions contained in his monograph prove the author to be intimately acquainted with minute anatomy, and with the microscopic structure of tissues, the engravings which accompany it, from drawings by himself, show that he is at the same time a first-rate artist. We have seldom seen a more beautiful specimen of anatomical drawing than is exhibited in the posterior view of the larynx, tongue, and epiglottis, given in Plate IV. The other figures, forty-nine in number, are equally accurate and ably executed; and amongst them are delineations of the final distributions of the nerves above described; of the microscopic appearances of true and false cartilage; of ossification occurring in cartilage; of elastic filaments; of the lamellated epithelium (a species of Henle's "pavement epithelium"), occurring on the mucous membrane without the cavity of the larynx; of the transition forms between this and the ciliated epithelium found from the upper third of the posterior surface of the epiglottis, and from a space three or four millimetres (·1181 to ·1574 of an inch) below the edge of the aryteno-epiglottidean ligaments; and lastly, of the fully developed ciliated or vibrating epithelium, found at the base of the epiglottis, on the ligaments of the ventricles, and on the posterior surface of the cavity of the larynx, to a height corresponding with these parts. On the ligaments of the glottis the pavement epithelium is again found; but immediately below them we once more meet the ciliated form, which is then continued uninterruptedly downwards.

The laryngeal glandulæ, which lie tolerably close in the sub-mucous areolar tissue, are found everywhere except on the ligaments of the glottis and the inside of the arytenoid cartilages, and are all acinous. Glandulæ of considerable size occur within the ventricles of the larynx, and, with their excretory ducts, present when magnified, an arborescent appearance, which is beautifully shown in Plate IV. fig. 1.

Traité Pratique sur les Maladies des Organes Génito-Urinaires. Par le Docteur Civiale. Troisième Partie. Maladies du Corps de la Vessie. Deuxième Edition, considérablement augmentée. Paris, 1851.

In this third Part of his valuable work, M. Civiale treats of the various diseases to which the bladder itself is liable,—tumours, abscesses in the walls of the organ, partial induration and

thickening of its parietes, hypertrophy, atrophy, ulceration, fungous growths, cancer, retention and incontinence of urine, hematuria, &c.,—a list much too extensive to be considered by us in detail. We deem it better, therefore, to confine our attention to a single one of the topics treated of, and the one we shall select is that of fungous tumours in the bladder.

A surgical subject may be interesting to the reader in two ways, either as being of everyday occurrence, and therefore of practical importance; or, on the contrary, from its rarity, being calculated to arouse curiosity. It is in the latter point of view that we present fungi of the bladder, for though they seem to be not very infrequent in France, they certainly are most rare in Ireland; and this, no doubt, for a reason mentioned by M. Civiale, namely, that they often accompany and depend on the presence of a stone in the bladder. Now as stone is rare in this country, it follows that fungi must be equally so; and this we believe to be the true reason, and not because they have been overlooked from their not having been sought for. Numerous examples of diseased bladder are preserved in the different museums in this city, and yet how very few specimens of fungi are among them.

M. Civiale's vast experience in diseases of the genito-urinary organs has led him to meet with many examples of these morbid growths, and he details their characters and symptoms with a severe, practical simplicity, suited to the gravity of the complaint. No other description equally full and satisfactory exists; we only fear that the treatment recommended by him for these diseases will be "*caviare* to the multitude," requiring that delicacy of manipulation,—that happy mixture of firmness and gentleness, which all who have witnessed M. Civiale's operations must allow him to be pre-eminently possessed of.

There has been some confusion among authors on the subject of fungi of the bladder, partial tumefactions of the prostate—its middle lobe particularly—having been mistaken for them. Morgagni, Chopart, Paletta, have fallen into this mistake. To avoid such error, M. Civiale confines the appellation of fungus or polypus to "tumours which spring from the internal surface of the bladder;" their variety is so great that it is difficult to lay down any general distinction of them. The neck of the bladder is their most common seat, but they are also found in different parts of its interior. "From some morbid disposition, but especially as the result of inflammation of the mucous membrane, there form excrescences, small soft masses, flabby, filamentous, spongy, susceptible of assuming the most

varied development and shape. In a practical point of view it is most important to establish the exact seat of the tumour, and from what tissue it takes its rise." In the early stages of those tumours, in which the extremity is larger than the base, as in polypi, M. Civiale has ascertained that they spring from the mucous membrane alone, traction of the little tumour drawing that membrane out by itself. When they have existed for some time, and have attained a certain size, their roots spread and sink deeper, and become mixed up with the other tissues, and form as a base a consistent mass, whose extent is generally, though not always, proportioned to the size of the excrescence.

When situated at the neck of the bladder they are more generally found to have a broad base. Sometimes a small tumour has a very long, thin pedicle; and M. Civiale, so long ago as 1825, met with a very interesting case of this kind, which he published, where the tumour, like a little cherry, was attached by a long and slender pedicle to the anterior face of the bladder, close to the vesical orifice of the urethra, which it often obstructed so as to hinder the flow of urine. This tumour was not discovered during life. However, even when small, the base is sometimes broad, even broader than the summit of the tumour.

"In the interior of the bladder we sometimes see granulations, or minute pediculated productions; the pedicle is nearly always very short. Then in the course of time, the tumour increases, it takes the form of a mushroom, and its pedicle enlarges. We then find that they adhere to the parietes of the bladder by a broad surface, although they belong to the category of those named pediculated. Thus, for the body of the bladder more frequently than for the neck, when the fungus has acquired a certain volume, it is fixed at the internal surface of the viscus by a sufficiently large base, and instead of only holding to the mucous membrane, as in the case of a slender pedicle, it buries its roots deep in the organ. These peculiarities are very important to be known, as special curative indications are deducible from them. The shape of the tumours varies according as they are pediculated or not. In the first case, we see in the beginning a small tumour, smooth, soft, more or less round, sometimes irregular on the surface, rough, mulberry-like, tuberculated, fringed, having its seat more especially round the internal orifice of the urethra, where it holds by a pedicle more or less long and nearly always slender. In other cases the pediculated tumours of the bladder resemble the little fatty excrescences that we remark on the integuments of certain very fat people. Some are flattened, others nearly triangular, and others resemble small cock's combs."

It would, however, be out of the question to enter into a

detail of the various bizarre shapes assumed by fungi when situated in the interior of the bladder. M. Civiale gives a plate in his second volume, where there are a number of filamentous fungi, fibrous tufts, with their ends floating in the bladder. They assume the appearance of warts, tubercles, or even of pustules. Whatever the shape in the beginning, it becomes curiously altered as the tumour enlarges. He has seen some perfectly conical, others irregular, knobbed, branching out, tuberculated, like mushrooms or cauliflowers. Their number varies no less than their shape. When they exist at the neck of the bladder the general rule is that the tumours are small; a very few instances of simple fungi are related by authors where the contrary has been observed. When they attain a very large size they are usually of a malignant nature.

As to the consistence of fungi of the bladder:

"Those which are small, round, pediculated, and situated near the neck, have little hardness; they only get hard after long continuance. Those which are mushroom-shaped are generally soft, especially at their free extremities, which are fringed, and often terminated by floating filaments. Some are so soft that, after having been seized by the lithotriteur, they are felt with difficulty, and the diagnosis is therefore uncertain."

With regard to the causes of their origin, M. Civiale, as we have already remarked, believes one of the most frequent to be a stone in the bladder, which keeps up a constant irritation in the mucous membrane. Other diseases having the same effect will equally tend to produce them. From the fact of such diseases being rare in women and children, fungi are much less frequent in them. Sometimes they present themselves without any apparent cause. The diagnosis of fungi of the bladder is most difficult, because their symptoms are the same as those of other diseases of the organ, particularly stone and disease of the prostate. Chopart says, and his opinion has been confirmed by all those who have since written on the subject, that he is ignorant of any symptoms by which the presence of fungi of the bladder may be known. M. Civiale thinks this difficulty is not exaggerated, having met with cases where, though the fungus had attained a large size, nothing had led to a suspicion of its presence, the most skilful practitioners having taken the disease for stone. A still more difficult question remains to be solved even after their presence is suspected, viz., whether they are of a simple or malignant nature.

Whether simple or malignant, while they are small there

are in general no indications of their presence; and afterwards, at a more advanced stage, when there are well-marked symptoms of disease in the bladder, those symptoms have no special character about them. Thus the difficulty of passing water, the interruption of the stream of urine, the vesical catarrh, the bloody or puriform matter, are all found in a great number of other diseases of the bladder. We may be assisted in our diagnosis by what the French call the method of exclusion; and M. Civiale enters into a most valuable and minute detail of those symptoms which may be considered peculiar to fungi of the bladder, and those common to other affections; but for these we must refer the reader to the work itself, which will well repay perusal. He concludes the enumeration of the symptoms by saying—

"The rational symptoms, of which I have just enumerated the principal, are not sufficient to diagnosticate a vesical fungus, even a simple one; but they are of such a kind as to arouse the attention of the practitioner, and determine him to have recourse to vesical exploration, the only means of establishing the diagnosis with certainty."

The exceptions are as follows:

"1. I have said that certain fungi of the neck of the bladder, or of its neighbourhood, may show themselves at the meatus urinarius in women, or in the wound of the perineum, or of the hypogastrium, after the operation of the stone.

"2. It is not rare to see in certain patients at the external orifice of the urethra fungous excrescences, which lead to the inference that some of a similar nature exist in the internal orifice of the canal. In a lady from Havre, who was under my care lately, this fact was evidenced in a remarkable manner. She was twenty years old, and suffered much in passing water. After having tried various medicines ineffectually, she came to Paris. The presence of a fungous excrescence at the meatus urinarius made me suspect others at the neck of the bladder: they were recognised and destroyed. Every morbid symptom disappeared.

"3. Portions of fungus become sometimes detached from the mass, as Colet has already observed, and come away in the urine or with injection, or are extracted by an instrument.

"4. It is not rare to observe in cases of well-developed fungi, and especially complicated ones, an alteration in the urine, which had been noticed by Cavillard and Colot, and to which I have for some time called the attention of practitioners.

"In most diseases of the bladder the urine has often a strong and fetid odour; but the fetidity is not that I wish to speak about. This fetidity has something quite peculiar. It is so penetrating, so repulsive, that it can hardly be borne. Cavillard says that 'the

people present were obliged to stop their noses,' and Colet assures us that the odour was so fetid 'that it was impossible for him to use the wig which had become impregnated with it.' When I observed this odour for the first time in the patient Inisan, my head, in spite of myself, was at once drawn back. In the patient Mondout the odour was less repulsive. The stone, phosphatic and very friable, was so penetrated by it that it infected my study in the space of a night. I was obliged to dry it outside."

This particular fetid odour, which is found in cases of cancer of the bladder, M. Civiale has perceived in fungi of the bladder, which had not gone on to the ulcerative state, and which he believes to be still curable. He cautions against the mistake of confounding small portions of whitened clot, or consistent mucosities in the urine, for the detritus of a fungus.

It has been already observed that fungus may be complicated with other diseases of the bladder, of the prostate, and even of the kidneys and ureters, and in such cases it is almost impossible to clearly distinguish their symptoms from those of the diseases which they accompany, the symptoms being in fact the same.

It is therefore to the information that is obtained by exploring the bladder that we must chiefly trust to diagnose the presence of fungi. This exploration is best done with lithotrity instruments, either the two-branched instrument of Heurteloup, of small size, or M. Civiale's three-branched instrument. Though this last in his hands may answer, as he believes, the best, yet we think that in Great Britain it will be little resorted to by surgeons in general. Whatever instrument is used, too much gentleness cannot be employed; the state of the prostate should be borne in mind, as considerable enlargement of the gland, particularly when it is accompanied by much irregularity of surface, is very apt to add to the difficulty of the examination.

We shall not enter into the various processes and precautions required in searching for the presence of a fungus in the bladder, because we feel that curtailment would materially impair the practical value of the directions of M. Civiale on this head. The reader should carefully peruse them in the work itself. We must follow nearly the same rule with respect to the treatment. So long ago as 1825 M. Civiale turned his mind to this object; in 1828 he publicly called attention to it; and in 1834 addressed a letter to the Academy of Sciences on some new methods of treating fungi of the bladder. His experience, therefore, is now fully matured. His first idea, when fungi with narrow pedicles were situated at the neck of the blad-

der, was to throw a ligature round the root; the difficulty of this manœuvre soon led to its abandonment. The next was to seize the fungus with a lithotrity instrument, and tear it away. Many small fungi have been thus removed, and the patients recovered perfectly. This mode is most applicable to those situated at the neck of the bladder. The same success does not attend when they occupy the interior of the body of the organ, and is totally inapplicable where their base is broad. When the disease is complicated with a stone in the bladder, this should first be removed, and then, after a time, the fungus should be attacked; sometimes, however, with the removal of the calculus, the irritation ceasing on which the fungus depended, the latter dwindles away, or ceases to be troublesome.

We may repeat, that these or any operative proceedings are only applicable to small fungi, and those especially which have a pedicle. When the tumours are very large they not only are unfit for the operation of removal, but they even offer resistance to the introduction of instruments, whether used with the view of exploration or of treatment. The treatment then reduces itself to removing as much as possible the obstruction which exists to the passage of urine, and to allay the irritation caused by the presence of the fungus.

We hope the reader will not be content with this brief abstract of the author's mode of managing fungi of the bladder. His detailed instructions abound in the most valuable practical details, which would be injured by being presented in disconnected extracts.

It is quite clear, as we have already observed, that fungi of the bladder are much less frequent with us than in France, but still they will occasionally present themselves; and it is when examining the work before us for assistance in their diagnosis and treatment that its real practical value will be felt. M. Civiale truly deserves well of the profession for having devoted the energies of his vigorous mind to the elucidation of a subject so obscure, but which his vast experience in urinary diseases renders him better able to do than any man living.

Pathological Histology[a].

We shall now resume the subject of the recent advances which have been made in medicine by the aid of the microscope, taking up the subject where we were compelled to break off in

[a] For list of Works reviewed see our last Number, p. 432.

our last Number, namely, with the new views in pathology which the investigations carried on by it have opened to the physician.

Professor Gluge's work will be found a very valuable addition to the micrologist's collection. Reduced from his larger and well-known Atlas of Pathological Anatomy, it contains, in the compass of one volume, a concise description and well-executed illustrations of the elements to be observed under the microscope in the principal pathological lesions.

After some introductory observations on the development of the histological elements in general, and the parallel between the physiological and pathological processes of cell formation, the author proceeds to consider the processes of nutrition, secretion, and inflammation. The several phenomena of inflammation he considers under the following heads: Congestion, hyperemia, stasis, exudation, and necrosis, which he regards as stages of the same process. In the fourth section we find some interesting observations on the metamorphosis of the blood, exhibiting views entertained for some time by M. Gluge, and in some respects differing from those of other authors. He considers that the blood can undergo distinct histological changes within the vessels, but in a more limited degree than when it is poured into the inter-capillary spaces. The blood-corpuscles unite together in groups of ten or more, which become subsequently surrounded by a cell membrane. This latter is gray, albuminoid in appearance, and soluble in acetic acid, when the red colour disappears altogether, and a cell is formed having various dark nuclei, which consist of fat or protein substance, or pigment. Sometimes, even before the cell membrane is formed, there appears in the centre of the mass of corpuscles a clear, large space, which is the nucleus of the cell; occasionally two such nuclei are formed. This metamorphosis of the blood-corpuscles into cells and *inflammatory bodies* (compound granular cells of authors), M. Gluge states cannot always be followed, the red colour rapidly disappearing, and these mulberry-like bodies only being visible; and he regards the existence of these bodies in the vessels as the only certain sign that a column of blood has been arrested in the process of circulation during life. The period necessary for the production of the changes he considers to occupy several days.

The nature of the process by which the blood-corpuscles pass into pigment granules is somewhat obscure. The corpuscles, at first red, become rust-coloured, and lastly yellow, or dark; their size is reduced, and the colouring matter is insoluble in either water or acetic acid, so that there appears to be a che-

mical change effected in it as well as in the protein. The fibrine contained in the vessels passes into fibres, at first flat and branched, subsequently rounded. Kölliker and Lebert maintain that these coloured bodies are not blood-corpuscles, but consider that they are formed anew from the hematin. In the blood escaped from the capillaries, similar changes take place, but on a more extensive scale, the corpuscles being changed into inflammatory globules, at first red, afterwards free from colour-cells and pigment granules; and cells are formed in abundance, the last (pigment cells) often containing granules insoluble in the mineral acids. It is scarcely necessary for us to add, that very many micrologists differ from M. Gluge with regard to the relation which those so-called inflammatory globules (compound granular cells of authors) bear to the inflammatory process. The remainder of the section is well worthy of attention; the characteristics of pus are well given; and the subjects of pyœmia and purulent diathesis are also briefly considered.

The subject of cyst formations is one of great interest to the pathologist, and of late much attention has been devoted to its study. In his Lectures on Tumours Mr. Paget describes three ways in which cysts may be formed:—Firstly, by the enlargement and fusion of the spaces of the areolar tissue; secondly, by the dilatation and growth of natural ducts; and thirdly, by the excessive development of cells or nuclei. The latter view has been greatly elucidated by the researches of Simon, Virchow, and Rokitansky; and we purpose making a brief extract from the labours of this last-named distinguished pathologist[a].

Cysts occur either single (having but one cavity) or aggregated (aggregated cystoid of Müller). Of these last, two forms exist: in the one, the mother cyst gives origin from some portion of its wall to a secondary cyst, from which, in turn, a third may spring. This variety generally springs from the outer surface of the primary cyst, whose walls are often brought into apposition by pressure. The second class comprises cysts in which the secondary growths take place from the inner surface. They are usually attached by a pedicle or neck, and assume a pyriform figure, often reaching very considerable dimensions. From the inner surface of both the primary and secondary cysts growths may be observed to spring, sometimes of a flat figure, sometimes mushroom-shaped or pedicled. These cauliflower growths may be either in

[a] See papers on the Anatomy of Bronchocele and on Cysts, by Professor Carl Rokitansky, in the Transactions of the Imperial Academy of Sciences of Vienna for 1850.

groups or united into masses, and present much variety in their size, shape, and arrangement. The size of the cysts is very variable, from an almost imperceptible vesicle to a sac 1′ to 2′ in diameter. The free space in the interior of the cysts is filled either with a serous synovial-like fluid or a thick glutinous so-called colloid matter. Under microscopic examination, the cyst wall is found to consist of thickly-woven areolar fibres. The inner layer is formed of an epithelium, consisting of either cells or nuclei. In large cysts this is often wanting, and the inner layer is then composed of a granular blastema, formless or striated, and presenting oval nuclei splitting into fibres, with their long axis directed inwards: incrustations of considerable size are often found in this situation. On examining the excrescences (intra-cystic growths), they are found to consist principally of a structureless hyaline membrane, beset with round and oval nuclei, and often striated at the pedicle. This membrane shows a tendency to split into areolar fibrillæ, inclosing spaces and terminating in rounded knots and loops, from which secondary loops pass off; they are either naked or covered with the epithelium of the cyst-wall. Vessels may be observed in these growths which, following the prolongations and loops just described, themselves form arches and loops. These vessels are frequently the seat of aneurysmatic enlargement, and thus give origin to the hemorrhagic effusions which so frequently take place into the cysts. Nuclei are formed in the excrescences in various quantity, and also young cysts. As this subject is followed out with great detail, we must refer to the original paper, contenting ourselves with abstracting a few of the more important conclusions at which Rokitansky has arrived.

The cyst is, in organization and secreting function, an independent hollow growth, whose essential foundation is a peculiar substantive element.

It constitutes a characteristic turning point between a primary (embryonal) and a secondary growth, essentially formed by a structureless tissue of a 25th to a 10th of a millimetre in diameter, and surrounded by a fibrous layer, whose elements are in different degrees of development, in addition to which there is an endogenous formation of nuclei or cells; and in this the cyst resembles completely the gland vesicle of the thyroid body, &c.

We shall next take up the diseases of cartilage. Before entering on the subject of disease in the articular cartilages, it may be well to consider briefly the most recent views as to their normal structure, which will be found very clearly enun-

ciated by Mr. Quekett, both in the Histological Catalogue and in his Lectures on Histology[a]. In the articular cartilage of the adult a white layer (the non-vascular lamella of Toynbee), varying in thickness and enclosing bone cells of more than twice the ordinary size separates the cartilage from the bone. The deepest layer of cells is disposed in rows parallel to the long axis of the bone, while the superficial layers are placed at right angles to the shaft, and are much flattened, and the inter-cellular matrix assumes a fibrous aspect in this situation. *Adult articular cartilage is not vascular;* the capillaries of the serous tissue of the shaft of the bone terminating in loops as they approach the non-vascular lamella. "The latest period of life," says Mr. Quekett, "at which I have discovered vessels in healthy cartilage is twelve years." In the fœtal condition, articular cartilage is fully supplied with blood-vessels which pass into its substance. In the fœtus each joint is covered with synovial membrane and an epithelium; "as soon, however, as the joint is used, the epithelium, and subsequently the synovial membrane itself, disappears from all parts subjected to friction, although, as Mr. Toynbee remarks, it is sometimes persistent on the patella. In all my observations on the synovial membrane, and in the sections made for description in the Histological Catalogue, which include specimens taken from a large number of animals of each of the four great classes, I have never found it very far in advance of the vessels[b]".

The capillaries of the synovial membrane are found to be disposed in a peculiar manner; the loops of vessels may be observed to pass on the cartilage as far as the part subject to friction; they do not enter the cartilage, and yet in Mr. Quekett's opinion there is no other source from which it can derive its nutrition. There is no proof whatever, that nerves enter into cartilage.

Having thus premised, we may pass to the study of disease in the cartilages of the joints. Much has been written on this subject hitherto, and by authorities of eminence, yet Dr. Redfern's labours will be found very acceptable to the pathologist, and his carefully detailed cases are very valuable. By reference to Observations II. and IV., it will be seen what an extensive amount of disease may exist over large articular surfaces without corresponding symptoms during life, and what changes may be wrought in joints of first-class magnitude without materially impairing the power of locomotion.

[a] Lectures on Histology, by John Quekett, &c. London: H. Bailliere. 1852. 8vo. pp. 215, with wood-cuts. Reprinted from the Medical Times, 1851.

[b] Histological Lectures, p. 149.

The general appearances of diseased joints have been already ably treated by authors, especially by Sir B. Brodie; it is, therefore, only in regard to the micrological characters of the diseased cartilage that we purpose availing ourselves of the researches of Dr. Redfern. The changes produced by disease invade both the cellular and the hyaline elements. In our author's observations some of the cells were found to measure a 100th of an inch in their long diameter, and others were five or six times their usual size, being rounded, oval or oblong, generally isolated, and irregularly distributed, instead of forming columnar groups. If the cells be only slightly enlarged, the nuclei are in small number and irregular, but if the cells be large, their contents differ entirely from those of healthy cells, consisting of a mass of corpuscles irregularly rounded, oblong, or triangular, glistening on the surface, and varying in diameter from a 2000th to a 5000th of an inch, the larger ones being finely granular or containing a small nucleus. These diseased cells often inclose others. The alterations in the hyaline substance consist of a splitting into bands and fibres, the smallest of which are not distinguishable from those of white fibrous tissue; these bands constitute the processes projecting into the joint, loose at one end and attached to the cartilage at the other. The velvety appearance so often seen on the surface of articular cartilage is likewise due to the fibrous degeneration of the hyaline substance.

Dr. Redfern's discussion on the pathology of this affection will not be found as well-worked out as the other portions of his observations. His meaning in some places is a little obscure, and one method of research appears to have been overlooked by him,—we allude to attempts to inject the diseased structure. He says, at p. 61,

"The essential nature of all the diseases of articular cartilages is shown by the preceding observations to be a change in the nutrition of their texture; that any one of these is produced mechanically cannot for a moment be entertained, after a careful examination of the texture of the diseased part."

Again, further on he observes, and, we shall see, without sufficient warrant for such a conclusion,—

"That the disease depends on actions going on in the cartilage and proper to it, is proved by its frequent occurrence without a vestige of disease in the synovial membrane or bone; and that all the changes go on independently of any peculiar action in the blood-vessels is shown by the appearance of the disease on the surface in by far the greater number of cases, and at parts far removed from the vessels of the bone, or of the synovial or other membrane."

More careful investigation has shown that vessels may be abundantly formed in diseased cartilage. In Plate VIII. fig. 11, of the Histological Catalogue, Mr. Quekett has delineated a section of diseased cartilage largely supplied with blood-vessels, and in his Lectures, p. 154, the following passage occurs: "The non-vascular character of healthy articular cartilage is maintained from the period of youth to old age; but if disease occur, vessels may be soon found in it. In a specimen from a diseased joint which after removal was carefully injected, numerous vessels may be observed passing through the cartilage; they are derived from the vessels of the shaft as the articular (non-vascular?) lamella, being involved in the disease, permits the vessels to pass through it; they proceed in straight lines through the cartilage to the free articular surface, upon which they form a network, and anastomose with others probably derived from the synovial membrane."

Mr. Quekett has examined "*the ivory of a porcellaneous deposit*," so common in chronic rheumatic arthritis,—a disease which has been so ably illustrated by the researches of our Dublin brethren, Mr. Adams and Professor Smith. A section, reduced to a sufficiently thin condition, was placed under the microscope, and found to be almost entirely destitute of Haversian canals, whereby the bone was rendered exceedingly dense; and he concludes that the new osseous matter having been prevented by friction from being thrown out upon the surface, was employed in filling up the canals, and, by this means, a substance originally porous was converted into a solid mass, susceptible of taking the highest polish. This view of the subject corresponds precisely, he says, with a practice adopted by French polishers, who are occasionally obliged to fill up the pores or canals in many of the hard woods, such as rosewood or mahogany, before they can bring the polish to any perfection.

The following are the practical deductions which Dr. Redfern draws from his researches. Some of them are exceedingly valuable, and in many essential particulars differ from the doctrines laid down by the classic authorities who have preceded him.

"1st. That the whole of the various forms of disease in articular cartilages are connected with changes in the texture, which are essentially similar to each other.

"2nd. That during the progress of these changes the cells of the cartilages become enlarged, rounded, and filled with corpuscles, in lieu of healthy cells, bursting subsequently and discharging their contents into the texture on the surface; whilst the hyaline substance splits into bands and fibres, the changed hyaline

substance and the discharged corpuscles of the cells, afterwards forming in many cases, a fibro-nucleated membrane on the surface of the diseased cartilage.

"3rd. That these changes are alone referrible to an anormal condition as their immediate cause, and in no case to mechanical or chemical action, such as attrition or digestion, in a diseased secretion.

"4th. That most extensive disease may be going on in many joints at the same time, and may proceed to destroy the whole thickness of the cartilage in particular parts, without the patient's knowledge, and whilst he is engaged in an active occupation.

"5th. That the disease commences most frequently upon the free surface; but it may proceed from the bone to affect the attached surface; or it may take place in the middle of the thickness of the cartilage.

"6th. That it is at least very doubtful if the symptoms which are believed to indicate the existence of ulceration of articular cartilages are not really dependent on a morbid change in the bone.

"7th. That disease of the whole thickness of an articular cartilage at particular parts admits of a natural cure by the junction of a fibro-nucleated membrane from the substance of the cartilage, without the occurrence of any new exudation."

A section comprising the results of experimental researches on the lower animals, and a sequel to the work, on the healing of wounds in articular cartilages, and on the removal of these structures, after amputation at the joints, &c., will well repay perusal.

We shall pass now to the subject of heterologous formations, commencing with the consideration of cancer, to the delineation of the elements of which we find two of M. Gluge's tables devoted, but we propose taking his observations conjointly with those of M. Lebert.

In the words of M. Lebert, cancer is a special malady differing from all others, and developed in virtue of a particular predisposition. As a tissue its fundamental character is, that it constitutes a veritable *substitution* of a new matter in place of the normal tissues in the midst of which it is deposited. It will be thus seen that M. Lebert is a most decided opponent of the doctrine of *transformation of tissues;* and we do not hesitate to state our own conviction that the balance of argument is most clearly against this erroneous doctrine. M. Lebert assumes the encephaloid or soft cancer as the typical form, and regards all the varieties of true cancer as referrible to the different proportion of the elements entering into their composition. The following are the varieties which he recognises:—The encephaloid, or soft cancer; the scirrhous, or hard; the gelatiniform,

or colloid; the vascular, or hematoid; and lastly, that possessing pigment, or the melanic. There are two remarkable alterations frequent in cancer, viz., hemorrhagic effusions; and the presence of a dull yellowish matter resembling tubercle, and which he consequently terms phymatoïd (*φῦμα*, tubercle). Without delaying on the subject of the physical characters of cancer—which are, however, ably treated and well worth perusal—we shall proceed at once to the microscopic elements. M. Lebert is, perhaps, the most strenuous advocate of the specific nature of the elements of cancer, and his observations are deserving of the highest consideration, both from the author's well-known character as an accurate and judicious observer, and from his large experience,—his opinions being now based on the results of examinations of more than 600 cases. We shall, therefore, make no apology for extracting at some length from his description of the diagnostic characters of the microscopic elements of cancer.

"If," says M. Lebert, "the following question be put to us: An isolated cell being given, can we always recognise by microscopic examination whether it belongs to a cancer or not? we will not hesitate to reply in the negative. But the question which we have always sought to determine is this:—A morbid tissue being given, can we recognise by means of microscopic inspection whether it is cancerous or not?" And on this point he does not hesitate to reply in the affirmative, with the sole reserve that there are certain exceptional cases, to be indicated afterwards, in which microscopic examination may prove insufficient, p. 16. Again, p. 17, he says:—"In proceeding after this fashion, that is to say, by the combination of clinical study and anatomical and microscopic examination, I have been able to separate from cancer a certain number of affections which had always been confounded with it. It is thus, guided by these observations, that I have been led to affirm in a clear and precise manner that it is possible to recognise in the great majority of cases, by microscopic inspection, whether a tumour be cancerous or not." We shall now present our readers with a condensed abstract of M. Lebert's description of the cancer cell. The cancer cell is often incomplete; and in many tumours we see a considerable number of large nuclei and nucleoli, and a proportionably small number of complete cells, a circumstance that is not to be observed in epithelial productions, nor in other tumours which may have some remote resemblance in their microscopic elements to those of cancer. When the wall is complete, the cell has a certain fixity of dimensions; and we may regard a diameter of $0^{mm}.02$

to $0^{mm}.025$ as the mean. If we compare these dimensions with those to be presently given for the nuclei, we arrive at this important conclusion,—that there is a particular proportion between the dimension of the cell and that of the nucleus, much greater than there is usually, in comparison to the wall which surrounds it; and this disposition is such that in many instances we may thereby recognise the cancerous nature of a tissue. "We have," says M. Lebert, "frequently examined, amongst others, cutaneous cancers which exhibited their cells with large nuclei and nucleoli; and if we compared with them the epidermis of the surface, and its large flat cells and small nuclei, we found such a difference that the least experienced observer could not mistake the one for the other." With regard to dimensions, the wall of the cell may have a diameter of only $0^{mm}.0175$; it may be even as small as $0^{mm}.015$, but these cases are as exceptional as those in which it reaches the diameter of $0^{mm}.03$, $0^{mm}.04$, and upwards. Besides this mean fixed for the dimensions of the cell, another not less important character is established, viz., the extreme variety of form of the wall. The typical cancer cell is, in the words of the author, a little, regular sphere, with an elliptical nucleus placed excentrically, occupying the half or more of the interior, and enclosing one or many large nucleoli. But this type is not often very pure; the cell wall may be oval, elongated, triangular, acute, or obtuse-angled, fusiform, or pointed at both extremities; at other times it presents, in place of two vertical appendages, a single long and pointed appendage, starting from a round cell, or three or four elongated pointed appendages passing off from an oval or irregular cell.

It would be impossible, and in fact useless, to specify all the forms: it is sufficient to note particularly that this *multiformity* of the cell wall is not observed to the same extent in any other kind of cell. If we examine epidermic tumours only superficially, we may imagine that there is a great variety; but we can always refer these forms to the different degrees of folding of the epidermic scale, or to alterations of various kinds, such as diffusion, or fatty or granular alteration. The contour of the cancer cell is generally pale and fine, and contrasts strongly with that of the nucleus, which is well marked. The cell contents, if there is no pigmentary or fatty-granular infiltration, are composed of fine, dull molecules, resembling the usual fibro (oleo?) albuminous granules. The pigmentary infiltration is usually a primitive variety, and not a secondary deterioration of the cell. In melanic cancer, whether of the smallest or largest volume, the pigment is found under two

forms, the more rare of which is that where it exists outside the cells; it is then in the form of fine granules of $0^{mm}.002$, or in little globules of $0^{mm}.004$ to $0^{mm}.005$; more rarely still in the form of the melanic globules of $0^{mm}.01$, devoid of nucleus and nucleolus, and entirely filled with pigment. Generally speaking, however, the pigment is contained in the cancer cells as granules or globules, which may exist in greater or less quantity, sometimes allowing the nucleus and nucleolus of the cancer cell to be visible, and sometimes so filling the cell as to make everything invisible except the pigment. Sometimes there may be observed a strange variety in the pigmentary infiltration, which is altogether limited to the nucleus. The pigmentary cancer cells are generally a little distended, and they may be observed to be larger by a fourth, a third, or even more, than the other cells of the same tumour. The nucleus of the cancer cell is, according to M. Lebert, the most constant element, but we must not hastily conclude the complete absence of the cell wall; thus, sometimes by the aid of very high magnifying powers, it may be seen that a pale cellular border surrounds the nucleus so closely that it could not be seen with low powers. This is an observation we can ourselves very fully confirm, as we have often experienced considerable difficulty in demonstrating the cell wall when more than usually delicate. Those who possess a microscope furnished with means for producing oblique illumination, either by any of the various prisms now in use, or by careful adjustment of the mirror, will do well always to have recourse to this method when any difficulty is experienced with regard to observing the cell wall or nucleus satisfactorily. We have thus often, by an oblique illumination, been able to study the characters of cells which were almost completely invisible by direct light. We may further take occasion to notice that we have met with many tumours in which the histological elements, both nuclei and cells, were of unusual delicacy, and required more than ordinary care and attention in bringing them at all into view; and yet in the general physical properties of such tumours any coincident characteristics, if present, have as yet escaped our research. The usual form of the cancer nucleus M. Lebert considers to be round when it is not completely developed; but the predominant type of the fully formed nucleus is ovoid or elliptical. Its borders are always strongly marked, indicating considerable thickness. The mean diameter is between $0^{mm}.01$ and $0^{mm}.015$. They frequently pass these dimensions, and reach $0^{mm}.02$. It is more rare to see them above $0^{mm}.01$ and $0^{mm}.0075$; and then, in the

majority of cases, the great volume of the nucleus and the multiformity of the cell wall can leave no doubt as to the nature of the product. In the normal state, certain pale granules may be found in the nucleus; but the chief element is the nucleolus, which is large, dark, and much more voluminous than in other kinds of cells; there may be one, two, or three nucleoli, but there are rarely more. It is unusual to observe the nucleolus of more than $0^{mm}.0025$, and its mean diameter varies between this last figure and $0^{mm}.0033$. It may be seen as large as $0^{mm}.004$ or $0^{mm}.005$, and three or more large molecules may be noticed in its interior. If we now take a summary view of the characters which are considered by M. Lebert as those special to the cancer cell, we shall find them to be:—1st, The relation of the nucleus to the cell as to size, the former having a mean diameter of $0^{mm}.02$ to $0^{mm}.025$, the mean dimensions of the latter varying between $0^{mm}.01$ and $0^{mm}.015$; and lastly, the nucleolus having a diameter of $0^{mm}.0025$. 2nd, The variety of form of the cell, indicated by the term *multiformity*, it being round, angular, caudate, &c.

It is greatly to be regretted that M. Lebert has not added a few illustrations to his present work; they would have materially enhanced its value, and rendered the conception of his views very much more satisfactory to the general reader.

Let us now see how these observations are borne out by the results arrived at by other inquirers. If we look to M. Gluge's delineations of the cell formations in cancer, we shall find that the groups of cells there figured agree in all essentials with the descriptions of M. Lebert. Thus we may note the proportionately large size of the nucleus, and the well-marked nucleolus.

Again, if we examine *seriatim* the wood-cuts which accompany Dr. Bennett's work, and which constitute such valuable studies when taken in conjunction with the recorded histories of his cases, we cannot fail to observe that in the great majority of instances, the characters insisted on by M. Lebert will be found present in those which can be strictly regarded as examples of true cancer. In addition to this testimony we may be allowed to remark that the results of our own observations (much more limited than those of either of the authors cited), confirm the opinions of M. Lebert, and we have no hesitation in stating our entire concurrence in the proposition of this author, already quoted, "that in the great majority of cases it is possible to recognise by microscopic examination whether a tumour be cancerous or not." It is needless to say how great a

boon will be conferred on the practical physician and surgeon if further research shall establish the truth of this opinion to the satisfaction of all; we do not deny that a good deal has yet to be achieved,—names of eminence are to be found in antagonism to these views,—and much accumulated observation will be still required before we can consider that the last hand has been put to this important work.

Besides the cancer cells just described, others are frequently to be met with. Of these the most remarkable are the mother cells, or cells including cells and nuclei; the latter may exist to the number of ten, fifteen, or more. M. Lebert is opposed to considering them as included cells, or examples of endogenous formation. Concentric cells are also to be met with occasionally, for a description of which, as also of certain other elements not unfrequently found in cancer, we must refer to the original work. It will be remembered that M. Lebert, in a portion of the observations which we have cited above, speaks of certain exceptional circumstances under which microscopic examination may not lead to the detection of cancer cells in a cancerous tumour. Such an exception occurs, he believes, only two or three times in a hundred cases, and is due to certain alterations which the cell undergoes, the principal of which are diffluence, thickening of the walls, diffusion, granular and fatty infiltration, desiccation, with deformity of the cells and nuclei. It may be observed, however, that these alterations never occupy the entire mass of the tumour, and that in the neighbourhood of cells which have undergone such changes true typical cells are generally to be found. Diffluence consists in the cells losing their contours and degenerating into a formless mass of molecular granules. This change may occupy, variously, either part or the whole of the cell; thus it may be confined to either the nucleus or nucleolus. Thickening of the cell walls is a sufficiently common change, the wall presenting double outlines, between which may be seen, sometimes granular matter, sometimes a dull, homogeneous substance It is in these cells that we meet with the completely hyaline nuclei resembling vesicles, an alteration well described by Virchow. Diffusion is the result of endosmose or imbibition, under the influence of which the wall becomes large and limpid, so that the cell appears like a vesicle with feebly marked borders. The granular and fatty infiltration is an alteration common to all cells, as Virchow and Reinhardt have shown, and which Lebert has observed even in the red corpuscles of the blood. The nucleus and nucleolus become completely concealed, and there

remains only a globular mass of granules. The desiccation and deformity of the cells is to be observed generally in atrophic cancers.

We shall not follow M. Lebert in the discussion of the objections raised to the specific nature of the cancer cell by Bennett, Vogel, and Virchow; his observations, to be appreciated, must be studied; we may, however, state as our own opinion, that the weight of argument is decidedly with our author.

Notwithstanding the confidence with which M. Lebert's large experience naturally inspires us, we are yet disposed to consider that in reference to prognosis in cancer his views are extreme, which we find evidenced in almost every section, as he omits no opportunity of insisting on the almost absolutely fatal result of the disease when it has once made its appearance. Thus at page 3 he observes: "We have said in our definition that cancer returned almost always after operation, and led to a fatal result,—we say, *almost always*, in order not to be too absolute." Again, at page 5:—

"The constancy of death in all the cases of cancer which we have been able to follow to their complete evolution is a fact at once striking and instructive; striking, because there is scarcely any chronic malady in which the same holds true in so absolute a manner; instructive, because this death occurs not only when there is manifest infection with numerous secondary cancers, but even when these depôts are not present, when the local malady does not produce accidents sufficiently grave to explain the cause of death; and we are obliged to admit, that there is a deleterious action on the entire economy, which it is not in the power of pathological anatomy to explain."

Similarly strong expressions of an extreme opinion as to the almost necessarily fatal course of cancer once developed in any locality will be found scattered through the work. Thus at page 79 we find the following passage:—

"We have met so great a number of cases in which there was neither sufficient suppuration nor repeated hemorrhage, nor very great functional disturbance which could account for death,—that we have been led to the conviction that the cancer killed, after a certain duration, by a general death, and not by a purely local affection,—by an infection of the entire economy, and not by accidental disturbance of some important organ. Cancer kills by producing in the blood an impossibility (incapacity?) of supporting life, and not by a deterioration of the nature of the blood, or of blood altered by exudation. We find proof of what we advance here even in cases of actual material infection; for this is often so insignifi-

cant, and occurs in parts of such little importance, that it is really insufficient to account for death. The re-action of secondary cancer, even when of a grave extent, is often so little apparent that, no matter what care is bestowed in studying the malady during life, an autopsy frequently establishes the existence of secondary deposits in organs where it was not at all expected to find them. . . . In a word, cancer, according to our experience, always leads to infection; and it is to the infection, and not to the local malady, that the fatal result is due."

Whether in these observations our author has not been led to outstep the strict limits of positive philosophic induction, we must leave our readers to judge. It must be familiar to all careful investigators of disease, that death frequently takes place without our being able to discover, by the most careful examination, sufficient to account for the fatal result, and this in other diseases as well as in cancer. One of the most interesting problems in the whole domain of pathology is that which embraces the question of death resulting from disease, whose mechanical effects are apparently insufficient to account for the cessation of life. We have still, in fact, much to learn in reference to the tolerance by the system, under certain conditions, of an amount of disease which even in the same individual may at another time rapidly produce death. And thus not only have diseases general and individual peculiarities in their lethal force, but it may also be observed, that very different powers of resisting the effects of disease are evinced by different individuals, and even by the same individual under different conditions. We may say of disease, therefore, that it possesses a certain lethal force, whose exponent varies within limits as yet unascertained; while the vital energy which enables the system to resist the shocks of disease, and which would appear to be something distinct from the physiological *vis vitæ*, inasmuch as the two are not infrequently in an inverse ratio, is also capable of being expressed by an exponent which varies likewise within limits hitherto undefined. What these conditions are, whereby death results without sufficient apparent cause in certain cases, it appears almost impossible to decide in the present state of science; but we are certainly not warranted in indulging vague speculations in the absence of any assignable cause; and the course which appears most consistent with the requirements of strict philosophical investigation is to register the results of careful *post-mortem* investigations, and to leave for a more enlightened era in medicine to solve the problem which now eludes us. Impressed as we are with these views, we cannot give in our adhesion to the doc-

trine of infection as the cause of death in cancer, thus absolutely propounded by M. Lebert.

Passing over several important sections, we shall take up that which considers the treatment of cancer, and more particularly that part which relates to the highly important question of operation.

Operation in cases of cancer, says M. Lebert, is in our opinion nothing but a palliative, which has for its object the diminution of suffering and the prolongation of life; and if the exclusive adversaries of operation complain that it does not cure, they unquestionably make too little account of the comfort which may be procured by it. Thus the prolongation of life for some months, and in fortunate cases even for one or more years, must not be lightly estimated. It is by no means rare to see patients after an operation resume their good looks, even though they had begun to become pale; and if it is certain that the local malady by propagation augments the chances of infection, we place the patient, who has been well and completely operated on, almost in the same conditions after each operation as the individual who is only predisposed, and who has not yet had a localization of cancer. Thus it is necessary above all to operate when we have the chance of being able completely to remove the affected part, when the malady is making incessant progress, and when it occasions considerable suffering. Scirrhous tumours offer less chance of return than encephaloid. We must abstain from operating when the disease is too extensive to be entirely removed; however, an operation may be had recourse to, if there is imminent danger of death unless it be performed; as, for example, in cancer of the back of the throat obstructing the digestive and respiratory passages. Operation must be abstained from when there are signs of a general infection, and when other tumours exist at a distance from the primary seat of the disease.

The operation once decided on, however, the fundamental rule is to remove largely, and to operate as much as possible in the sound tissues. Too much attention cannot be bestowed on the careful examination of the bottom of the wound and the neighbouring parts after the removal of the disease. But if it be well to remove freely and completely, it is necessary, as far as possible, to unite the borders of the wound, and to try to effect union by by the first intention. The author further states his opinion, that it is most certainly a prejudice without any foundation whatever to believe that there is a salutary and preservative influence as regards relapse in the continuance of a long suppuration in the wound. With regard

to dressing, M. Lebert is of opinion that the first dressing should be removed on the first or second day after the operation, and more especially in case of an operation on the breast, always, however, leaving the bandages in their place. During the first and second days which follow an operation there is such an abundant exhalation of serosity, without taking into account the blood which escapes, that the dressings are quickly saturated, aud keep the part in a disagreeable and injurious state of moisture. And we should not be astonished, says the author, if, after the operation for cancer of the breast, the removal of the dressings on the fourth day, of itself alone predisposed to pleurisy by propagation, which constitutes a frequent and dangerous accident attendant on this operation. It is necessary, moreover, to bear in mind, in hospitals above all, the chances of mortality as a consequence of the operation itself. Under this point of view, amputation of the limbs for cancer offers the same unfavourable chances as amputation in general. Operation of cancer of the breast furnishes in the great hospitals of Paris about one-sixth of the mortality; that which results from operations on the testicle, the eye, and the different other external organs, is much less considerable. There is also another consideration, namely, that for the surgeon who does not distinguish cancroid from cancer, the necessity for operating is much greater. For our part, says M. Lebert, we carefully distinguish these affections, and we are of opinion that we can dispense with operation as long as cancroid diseases remain stationary and strictly localized. It is useless, for example, to remove a small benign tumour of the breast which has already existed many years without inconvenience; but as soon as the disease begins to extend, it is necessary to operate freely; and as local relapses are only in this instance the continuation of the primary disease, it is necessary to follow them up with perseverance by practising operations more and more extensive, for it is in these cases that two or three operations bring about a cure which could not be obtained by a single one. Must we operate in cases of relapse of true cancer? To this question our author answers, that the precepts just given are fully applicable; and says, if we take cancer of the breast where relapses are so frequent that in two-thirds of our observations they have occurred at the end of six months, we find that in the remaining third they did not take place until the end of the first, second, or even third year. Thus, if these latter patients have enjoyed a real benefit from the first operation, they may still gain, after the second relapse, a period of a year or two; and as

these cases are not too exceptional, they still offer a chance of safety, if not permanent, at least transient. As to the constancy of relapse in true cancer, M. Broca and I, says M. Lebert, have been able to establish it in all the cases which we have been able to follow during sufficiently long periods. For ourselves, we may state, as we have said before, we have a faith and a hope, that extended and careful researches will prove this rule to be somewhat too absolute; and we therefore concur entirely in the author's observations with regard to operation under suitable circumstances, even in cases of true cancer. Some other modes of treatment which have been more or less vaunted are considered, but, in the opinion of M. Lebert, experience does not warrant our placing that confidence in them claimed by their respective supporters; and, amongst other modes, he states this to be true also of the treatment by pressure, whose beneficial effects have been so much insisted on by M. Recamier. The entire of this section deserves careful perusal, and we earnestly recommend it to the closest consideration of the operating surgeon.

There is no subject more carelessly dealt with by the majority of medical practitioners than that of the statistics of disease. Few, indeed, can be considered to be at all well informed as to the data which actually exist for estimating the numerical results of any particular disease or operation; and yet no criterion is more confidently appealed to in all cases of doubt. It is only on careful investigation of any particular branch of vital statistics that we become fully aware of the lamentably deficient state of this branch of medical science; and even in instances where numerical tables have been brought forward with much parade, we too often find, on submitting them to a thorough analysis, that they only bear intrinsic proof of their own inaccuracy, and show the untrustworthy nature of their results. There is no portion of the statistics of disease to which these observations apply more fully than to the questions connected with the frequency of the occurrence of cancer in the several organs of the body, and the result of operations undertaken for the removal of cancerous tumours. The almost general omission of all attempts at distinguishing true cancerous disease from cancroid and other affections which bear a certain resemblance to it, is alone sufficient to vitiate the greater number of numerical observations which have been hitherto made. Thus M. Lebert demonstrates the striking inaccuracy of the Report published by Tanchou, which embraces 9118 cases, and which he states does not warrant us in drawing any conclusions whatever. Under the able superintendence

of M. Marc d'Espine, of Geneva, a table has been constructed on the carefully recorded observations of seven years. The comparative frequency of cancer in the several organs, as shown by this table, is as under:—

Cancer of stomach, . .	209	Cancer of œsophagus, . .	8
,, uterus, . . .	72	,, mouth, . . .	6
,, liver, . . .	59	,, genital organs in man, . . .	5
,, breast, . . .	44	,, bones, . . .	4
,, digestive tube, .	14	,, indeterminate seat,	19
,, glands of neck, .	12		
,, rectum, . . .	10		471
General cancerous diathesis, ...	9		

Much difference will be observed with regard to certain localizations of the disease between the results of this table and one constructed by M. Lebert, based on his own observations of 447 cases. Thus cancer of the breast stands at the head of the latter list, being marked 62, while cancer of the stomach and uterus are respectively 57 and 52.

The second part of M. Lebert's work is occupied with the special history of cancer and the maladies confounded with it, as they occur in the various organs of the body; and we find two sections devoted to the study of the pathology, anatomy, history, and diagnosis of this malady as it is to be met with respectively in the organs of generation, the organs of digestion, the skin and subcutaneous areolar tissue, the superficial glands, the osseous system, the nervous centres, the eye, the urinary passages, and the organs of respiration and circulation. To those investigating cancer in its occurrence in these several localities this work will be an invaluable assistance. Our limits will not permit us to consider more than one or two of the more important sections.

It is of the highest importance to distinguish the true carcinomatous affections of the uterus from all those which bear such an amount of resemblance as to be capable of being confounded with them on superficial examination. With the assistance of the microscope, such errors in diagnosis may, in the great majority of cases, be altogether avoided; and when this instrument becomes familiar in the hands of the careful clinical observer, we anticipate that still fewer mistakes will occur. The diagnosis between the cancerous and cancroid affections of the uterus is not a matter of solely scientific interest: many practical questions are directly dependent on it; for, as remarked by our author, while on the one hand the difference of structure implies a pathological difference, on the

other we may flatter ourselves with the hope that, as the cancroid affections are more strictly local than true cancer, we shall succeed one day, when these diseases shall have been better studied, in affording more active assistance than can be done in the actual state of science at present.

The following is the classification of the cancerous and cancroid affections occurring in the uterus which M. Lebert adopts:—

A.—True cancer, which is found of three forms:—

a. As a soft infiltration deposited at first in the midst of the elements of the vaginal portion of the neck, and which thence extends little by little to the body of the organ, and which may go through all its phases (as afterwards shown) without ulcerating the neck, though this last termination is the most frequent. This infiltrated cancer may be either encephaloid or scirrhus, and the ulcer resulting from it is anfractuous, *rongeant* in part of its extent, and covered with bossy vegetations in others.

b. It is not rare to meet in the neck of the uterus the vegetating form of cancer, which from an early period separates and distends the elements of the neck, and terminates by forming cancerous growths, which invade a large part of the vagina, and tend always to increase: they are soft, yellow, infiltrated with an abundant cancerous juice, of a rosy tint in vascular places, and of a deeper red when vessels are very numerous, which gives them the character of fungus hæmatodes.

c. The third form is colloid cancer.

B.—Cancroid of the uterus shows itself under the following forms:—

a. An eating ulcer, with absence of cancerous tissue in its base, in its borders, in its surface, and in its neighbourhood. It is true, says the author, an inattentive observer might sometimes mistake the indurated and callous tissue in the neighbourhood of the neck for scirrhus; but the absence of infiltrating cancerous juice, the insensible passage into sound uterine tissue, and, above all, microscopic examination, will be sufficient to remove any doubts. This last examination (microscopic) will show in these ulcers and in their neighbourhood the elements of inflammation, of suppuration, and the necrosed detritus of the uterine tissue. Notwithstanding the absence of true cancerous tissues, these ulcers tend continually to increase in size and depth, and thus bring about almost the same grave accidents as the cancerous ulcer.

b. If the phagadænic ulcer just spoken of has been already

noticed by Clarke, Rokitansky, and other observers, I believe, says M. Lebert, that I was the first to recognise the nature of the vegetating epidermic cancroid of the uterus. The most grave form of this kind of tumour is that which consists in an exuberant vegetation of small papillary tumours, occupying all the fundus of the vagina and the neck, and which, in its *ensemble*, presents a grumous consistence, with absence of all infiltrating succus, and a leaf-like consistence,—which may be recognised by mixing the tissue with water, to which it does not communicate the troubled and lactescent appearance of the cancerous succus, but in which the groups of leaves become separated from each other, leaving the water in the intervals transparent.

c. The epidermic ulcer, although having a sufficiently destructive tendency, is distinguished from the primitive eating ulcer (*rongeant*) in that its surface and border present papillary and epidermic vegetations of the nature of those just described. Another variety consists of small, hollow, multiple ulcers, containing a yellow grumous matter, which under the microscope appears entirely composed of epidermic scales. This is the kind of ulcer often mistaken for a tuberculous affection of the cervix, to which, in fact, they have a certain external resemblance. M. Lebert, therefore, proposes for them the name of phymatoid epidermic ulcer (φυμα, tubercle), and he thinks it probable, from recent remarks made on the mucous membrane of the cervix, that they originate in a primitive affection of the glands of the cervix, which become dilated and filled with epidermic elements.

We are here obliged to pass over several very interesting sections, in which the state of the cervix is fully considered, in order to make a few extracts from our author's observations on *the microscopic characters of cancer of the uterus.* The fibrous stroma of scirrhus offers the same filamentous hairy appearance as elsewhere. The cancer cell, the only truly characteristic element, presents the following aspect:—The form of the cell is very variable, sometimes round or ovoid, sometimes triangular, often irregular and elongated, and sometimes terminating in a fine thread; the cell wall is often completely wanting: the mean dimensions vary within the limits before assigned (see page 181). The nuclei are in cancer of the uterus, as elsewhere, an essential element, and exist, often in a predominant number, in proportion to the cells. Their shape and dimensions are much the same as in other specimens of cancer, and this holds likewise with regard to the nucleoli (see page 182), as before. Fibro-plastic elements are almost

constantly to be met with in cancerous tissue of the uterus, which is the less remarkable, since M. Ch. Robin has ascertained that they exist in the mucous membrane of the uterus in the normal state. Fatty granules and vesicles are constantly to be met with, but melanic elements, either as stains, granules, or globules, are rare, occurring only twice in the forty-five cases observed by M. Lebert. The elements of pus are generally but incompletely formed. As to what concerns the microscopic study of the surface of ulcers, it shows, besides the cancerous and uterine detritus cells of epithelium, altered globules of blood and pus, and sometimes crystals. The colloid tissue exhibits under the microscope an almost transparent amorphous tissue, occupying the interstices of a fine stroma of fibres, and enclosing numerous granular bodies. As to the papillary and epidermic tumours, pavement or cylindrical epithelium is to be found in them, the former generally in greater abundance. It is the ovoid, or irregular leaves, often offering a folded appearance, which give them a fibrous aspect when they are placed in close juxtaposition. Their diameter varies between $0^{mm}.02$ and $0^{mm}.04$; they enclose a small granular nucleus of $0^{mm}.005$ to $0^{mm}.008$ at most, rarely provided with true nucleoli; in the centre of the papillæ a fibrous tissue is found. With regard to the structure of the little papillary tumours, designated as hypertrophied and vascular eminences of the mucous membranes, the microscope exhibits in them venous and arterial nets, and the stroma of the mucous membrane, covered by an epithelial layer. If we examine the cancroid ulcers, the microscope shows the ordinary elements of ulcers, with an absence of cancer cells; epidermic scales are present when the ulcer is of epidermic origin; these, however, are absent when we have to do with a simple eating cancroid ulcer.

After detailing the microscopic appearances, M. Lebert proceeds to study in the fullest manner all the changes which are produced in the cancerous uterus and the structures in connexion with it, both immediately and remotely. These, as well as the whole question of general and special symptomatology, are all, however, subjects rather for actual perusal by the practical investigator of the disease than for abstract in the pages of a periodical. It is our object principally, by noticing the results of this most laborious inquirer, to invite a close study of the work itself. Under the head of progress and duration we find the following observations:

" The cases are equally rare in which the progress of the disease is very rapid, and those in which it is very prolonged;

it is only in exceptional forms that cancer of the uterus assumes the acute form. We have already cited an observation of M. Louis, in which the total duration was under three months. M. Kiwisch (Von Rotterau) has related to us an instance in which five weeks formed the entire duration of the disease; the affection supervened on an accouchement which had been natural and easy; and as the author, M. Kiwisch, had himself been in attendance, he was able to affirm the absence of all cancerous affection at that period." As regards cancer of long duration, M. Lebert has seen but one case which lasted so long a time as five years and two months. He throws doubt on the precise nature of a case cited by M. Louis, in which the duration of the disease was stated to be above nine years.

The results of 39 cases observed by himself are as follows:—In nearly one-third the duration did not exceed nine months; in another third, the duration was between nine and eighteen months; and in the remaining third it was above eighteen months. Of the last division, 7 cases were of two years' duration, 2 of twenty-three months, and 3 above two years. The mean of these 39 cases gives sixteen months and 7-39ths, which is nearly five months less than the mean duration of cancer of the stomach, which itself falls short a good deal of that of superficial cancers. The result of M. d'Espine's researches gives 83 cases of death from uterine cancer, amongst 537 cases of mortality from cancer in general, during a period of eight years, in Geneva. The annual mean of general cancer is, according to the same authority, 67, of which 10 cases are cancer of the uterus, or in the proportion of something between 1-6th and 1-7th, a frequency very much less than that given in the tables of M. Tanchou. The accompanying table shows the distribution of 50 cases according to the ages of the patients:—

From 25 to 30 years of age,	5	From 50 to 55 years of age,	3
„ 30 „ 35 „	5	„ 55 „ 60 „	5
„ 35 „ 40 „	9	„ 60 „ 65 „	3
„ 40 „ 45 „	8	„ 65 „ 70 „	3
„ 45 „ 50 „	8	„ 70 „ 80 „	1
		Total, ...	50

The mean in these 50 cases is 44 and 2-25ths. Our author throws much doubt on the diagnosis in the cases related by Boivin and Duges as occurring before the age of 20. With regard to the causation of cancer of the uterus, much importance has been laid on the influence of fecundity, hereditary predisposition, grief, profession in life, anterior state of health, occurrence of syphilis or scrofula; but, in the opinion of M. Lebert,

there do not exist grounds for believing that there is any connexion between cancer and any of these conditions as cause and effect. In reference to the question of fecundity, the statistical investigation of 37 cases gives the most complete refutation to the common assumption. Thus, in those having only one child, 8 cases occurred, and in those having from seven to thirteen children but 1 case. The period of time elapsed between the last accouchement and the appearance of the malady has also been noted with care. With the exception of 3 cases, of which 1 occurred at a distance of ten months, and 1 at four months from the accouchement, the third being developed during pregnancy,—in all the remaining cases the commencement of the disease was separated from the last accouchement by from two to three years at least, and in the great majority of cases by a much longer lapse of time. These results will give an idea of the accuracy and detail with which all the questions relating to cancer of the uterus are treated. We have not alluded to the subject of treatment, but the chapters on this topic well merit perusal. A very valuable *resumè* of the entire will be found from pp. 306 to 317 inclusive, to which we specially direct the reader's attention.

Passing over the sections devoted to cancer of the ovaries, we come to the consideration of cancer of the mammary gland, the author's conclusions on this subject being based on sixty-two observations. As in other situations, he recognises but three varieties of cancer occurring in the breast,—the scirrhous, the encephaloid, and the colloid, of which the first form was met with in three-fifths of the cases, the second in one-fifth, while in the remaining fifth the character of the tumours was intermediate between scirrhous and encephaloid, constituting the "*elastic*" form of this author; the true carcinomatous colloid was found in only one case. We have not space to make any abstracts from the chapters devoted to the general history, pathology, and micrology of cancer of the breast; we pass at once to the statistical results.

"It is not only for the sake of prognosis, says M. Lebert, but also for the sake of determining the question of treatment, that it is essential to know accurately the progress and duration of cancerous affections." It is really astonishing that no good statistics exist on this subject. In the present work we have data from the progress and duration of 59 well-observed cases of cancer of the breast: of these, 27 were noted up to the period of death. Of 34 cases, in which the duration of the disease was ascertained at the period of the first operation, 5-7ths presented a duration of from three months to two years, and

2-7ths a longer period. With regard to relapses after a first operation, and the interval which separated the second from the first operation, we have the results of 21 cases. In 2-5ths of this number a second operation was required in from one to three months after the first; 6 patients underwent operation a second time in from three to six months; 4 at the end of from one to two years; 2 at the end of two to three years; and 1 only at the end of eight years. It is necessary, says M. Lebert, to make a certain restriction in favour of the operation, of which he is by no means an antagonist. He has observed in some rare cases that relapses did not take place until several years after the first operation, when this was required at the end of from six to nine months after the first appearance of the tumour; whilst, on the contrary, relapses took place promptly in patients who had borne cancerous tumours of the breast for many years. Nothing, in our opinion, could constitute a more forcible argument for the necessity of strict and accurate diagnosis and early operation, when existing circumstances admit of it. In 5 cases the results of a third operation for second relapse are noted: 3 in the space of one to three months after the second operation; 1 in the space of three to six months; and 1 in the space of six to nine months. The last series of facts adduced by our author which we shall notice are those which relate to 19 cases in which no operation was performed. Of these, 6 still lived at the date of publication; in 2 the duration was from one to two years; in 1 from two to three years; in 2 from four to six years; and in 1 twelve years. Amongst the 13 who had died, the duration of the disease was in one case from three to six months; in 6 from one to two years; in 3 from three to four years; and in 1 twelve years. All these results are given in two synoptical tables (p. 353), for which we must refer to the work itself. The following observation of M. Lebert deserves most careful consideration:—

"Without wishing to draw more conclusions from these statistics than they actually warrant, we can decidedly affirm that the total result of operations has not been happy, and that they have not exercised any manifest and certain salutary influence on the mean duration of the disease; a result which ought to make us very reserved in the choice of operation, without, however, leading to its exclusion in an absolute manner."

No subject requires more careful study, or the exercise of more accurate diagnosis, based on all the methods of research now within our reach, than that of the diseases of the mamma capable of being confounded with cancer. Of these, the most

remarkable is the partial hypertrophy of the mamma, to which a very considerable section is devoted by our author. After a brief outline of the normal anatomy of the gland, he proceeds to consider the pathological anatomy of this condition: we shall content ourselves with an abstract of the observations on its microscopic structure. With low powers (20 to 40 diameters) we may often recognise the presence of the primitive elements of the gland, the terminal *culs-de-sac*, either isolated or in groups of two or three; they are sometimes in the form of grapes or of an oak-leaf. Their form is generally rounded and ovoid, sometimes lancet-shaped, at others slightly notched, their mean size is 1-5th to 1-12th of a millimetre. Ordinarily the anhystic membrane which limits their primitive lobules externally is only visible by its perfectly defined outlines. It may, however, be thickenened considerably. The most interesting and essential portion of these tumours is the epithelium which clothes the internal surface of these lobules; it is sometimes in considerable quantities without our being able to see the lobules themselves. This epithelium is ordinarily rounded; it may, however, occur of a cuneiform or cylindrical shape. The complete cells have usually a diameter of $0^{mm}.01$ to $0^{mm}.012$, occasionally reaching $0^{mm}.015$,—by reference to the measurements given at p. 182, it will be seen that these now given correspond very closely with those of the cancer nucleus;—they are provided with a round or ovoid nucleus, varying between $0^{mm}.005$ and $0^{mm}.0075$; a great number of these nuclei occur without envelopes; they enclose punctiform nucleoli of $0^{mm}.0012$ to $0^{mm}.0015$. These cells are frequently met filled with fatty granules, which mask the nucleus and distend the cell walls, giving them a size which may reach $0^{mm}.02$. The aggregation of these fatty globules gives to the lobes a dull, yellow tint; numerous crystals of cholesterine are also found. We may observe all the intermediate degrees between a simple membranous expansion in the interior of the lobules, and an aggregation so considerable that the lobes are completely filled with them. At other times, on the contrary, the fibrous element becomes so developed that the epithelium disappears almost entirely.

Partial hypertrophy of the mamma does not usually return after operation; and even should there be a relapse, a second operation, or still more rarely a third, brings about a complete cure; on the other hand, if the tumour be not very extensive, and contains no cysts, it may persist during the entire life of the individual without any grave consequences. The following table gives the duration of the disease in 23 cases up to the date of operation:—

From 6 months to 1 year,	6	From 6 to 7 years,	1
„ 1 year to 2 years,	2	„ 7 „ 8 „	3
„ 2 „ 3 „	3	„ 8 „ 10 „	3
„ 3 „ 4 „	3	„ 10 „ 12 „	3
„ 4 „ 5 „	2	„ 12 „ 13 „	1
„ 5 „ 6 „	2		
		Total,	23

With these abstracts we must conclude our notice of the sections on cancer and cancroid of the mamma.

Cancer of the testicle, of the palate, tonsils and pharynx, the tongue, œsophagus and stomach, the intestines, liver, spleen, pancreas, &c., are next considered in separate sections. We can only indicate these several subjects to the reader, as we are anxious to make some extracts from the chapters on cancer and cancroid of the skin, and more especially the latter. The following is the classification of tumours of the skin adopted by the author:—

"1st order. *Epidermic tumours.*—This variety of tumour consists in a hypertrophy of the superficial layer of the epidermis, or of the deeper papillary layer.

"2nd order. *Local pigmentary development in the skin.*—These constitute the coloured erectile nævus, which it is necessary to distinguish carefully from melanic cancer, which is nothing but encephaloid very rich in black pigment.

"3rd order. *Circumscribed hypertrophy of the sub-epidermic vascular layer.*—This order comprises the erectile tumours; they are often accompanied by a new formation of areolar tissue, more or less abundant in the interspaces between the dilated vessels.

"4th order. *Glandular tumours of the skin,* which comprise engorgement and hypertrophy of the sebaceous glands of the dermis.

"5th order. *Hypertrophy of the dermis.*—This order comprises the elephantiasis of the Arabs, &c.

"6th order. *Cancer of the skin.*—Cancer of the skin differs from all the preceding orders of tumour in the formation of a new element, foreign to the organism,—in a word, heteromorphous."

All the forms of true cancer, scirrhous, encephaloid, colloid, and melanic, are to be met with in the skin. We shall pass at once to the subject of cancroid of the skin, as in cancer in this organ the diagnosis is to be made, as elsewhere, by the aid of the microscope. Under the head of cancroid, our author ranges those affections designated by others as verrucous cancer, chimney-sweep's cancer, cancrous ulcers of the face, noli me tangeri, &c. Cancroid of the skin may assume various forms; it usually commences as a hypertrophy of the superficial layers, and we may distinguish three varieties:—1st, That which has

its seat in the proper epidermic layer; 2nd, That of the papillæ; 3rd, Epidermic hypertrophy, with infiltration of the dermis to its deepest layers. These three forms are often combined; but of them all the third is the most grave, because the epidermis tends to become infiltrated by little and little, until it reaches the muscles and bones, and infects the neighbouring lymphatics and glands. The following table shows the order of frequency of cancroid in different situations in eighty-one cases:

Cancroid of the lower lip, .	20	Cancroid of the vulva, . .	4
„ „ face, . .	22	„ „ different regions,	10
„ „ penis, . .	9	„ „ uterus, . . .	7
„ „ tongue, .	4	„ „ dura mater, . .	1
Keloid, ...	4		—
		Total,	81

No case of cancroid of the scrotum has occurred to M. Lebert; his descriptions are, therefore, drawn from those of English authors.

The natural structure in the integument which becomes the seat of this disease is the epidermis, which varies in thickness between one-tenth and one-fourth of a millimetre. It is composed of layers of cells imbricated and superposed, the most superficial being dry, and forming irregular scales, generally destitute of nuclei; but as we examine the deeper layers of the epidermis, more characteristic cells are met with, varying in size between $0^{mm}.02$ and $0^{mm}.03$; their nucleus generally varies between $0^{mm}.005$ and $0^{mm}.0075$; these dimensions of the nucleus of the epidermic cell, contrast very distinctly with those of the cancer nucleus, which, it will be remembered, varies between $0^{mm}.01$ and $0^{mm}.015$; this diagnostic character deserves to be constantly borne in mind. The nucleus of the epidermic cell is frequently surrounded by a transparent zone, which is probably only a second envelope. It is only in very young epidermic cells, and in those which are found in the hypertrophied products of this tissue that nucleoli of $0^{mm}.002$ in diameter, and one or two in number occur in the nucleus. The papillæ of the dermis are but fibrous cones surrounded by an envelope of epidermis; in the natural state their dimensions vary between one-fifteenth and one-tenth of a millimetre.

In cutaneous cancroid tumours all the transitions between the most innocuous and the most malignant appearances are to be found. If we take an ordinary wart, we observe under the common epidermic envelope a number of the papillæ increased in volume, sometimes reaching one millimetre in length: as many as from six to eighteen may be found in one wart. In the

salient and mulberry-form of cancroid tumours, precisely the same elements will be found. The most important and grave form of this affection is that in which the hypersecretion of epidermis is not confined to the papillæ and the superficial layer, but also surrounds their base, and extends to the deepest layers of the dermis, the fibrous strata of which become completely infiltrated, assuming a pale yellow homogeneous, grumous aspect, sometimes filled with a thick substance resembling glazier's mastic, which, when mixed with water, becomes separated into leaflets, without communicating to the fluid the troubled appearance which results from the admixture of cancerous succus or pus. This kind of epidermic infiltration, accompanied by papillary hypertrophy, may invade all the subjacent textures, muscles, bones, and lymphatic glands. Cells in all stages of development are found in this kind of growth; they may even become filled with fatty granules. Another element, which M. Lebert says he has never found in any morbid product except cancroid of the skin, tongue, and cervix uteri, are the "epidermic globes," composed of a concentric arrangement of epidermic scales, which towards the periphery are so closely apposed that the globes assume a truly fibrous aspect: these globes vary in size from 1-20th to 1-10th of a millimetre, and upwards. An important practical fact connected with cutaneous cancroid in general is, that the surrounding portions of skin are infiltrated with epidermic elements in a diffuse manner, which greatly facilitates relapse when the operation for removal has not been sufficiently extensive. We may inquire how the epidermic infiltration reaches the glands which are in direct anatomical relation with the primary cancroid, beyond which, in the opinion of M. Lebert, it never reaches either to distant glands or other parts of the body. We may suppose that there is a direct transport of the epidermis through the eroded lymphatics. One of the most important points for an appreciation of the nature of cancroid is the mode of participation of the entire economy in the local disease. "*We can affirm*," says M. Lebert, "*in a clear and positive manner, that we have never observed secondary cancroid disease beyond the circle of direct propagation of the primary local disease.*" As the result of an analysis of eighteen *post mortem* examinations, he states that in all there was a total absence of secondary deposits similar to the primary disease; whereas in true cancer, general infection of various organs was found in from one-third to two-thirds of the cases. The following passages sum up, briefly but distinctly, the analogies and differences between cancroid and cancer.

1. *Analogy.*—Ulcerating and invading tendency; possibi-

lity of infection of neighbouring glands, and of fatal termination by exhaustion and putrid infection, when nothing has been able to stop the progress of the cancroid ulcer.

2. *Difference.*—Different structure and aspect; microscopic composition quite different from that of cancer; finally, absence of general infection and of multiple and distant deposits in the entire economy.

To our minds no further evidence can be required to show the marked line of distinction which separates cancroid disease from true cancer. Their diagnosis becomes of the highest possible importance, especially when we come to consider the great questions of prognosis and treatment. The latter subject remains now to be considered; and first, with regard to removal by operation. "Extirpation," says M. Lebert, "appears indicated when we can remove the cancroid tumour in a large extent, and when the operation can be performed in sound structures, far beyond the suspected portions, and far beyond that portion of skin which, without being itself affected, may be suspected of being so from its very contiguity to the disease. We must not pause at the extent or depth of an operation. We must remove muscles, bones, and lymphatic glands, and even, if necessary, perform amputation of a limb. In a word, we must operate largely and early." With regard to the form to be given to the portion of skin to be removed, M. Lebert is decidedly opposed to all exclusive rules. He is convinced that adherence to the V-shaped incision for cancroid of the lip is one of the causes of relapse in this situation; and he much prefers a rounded incision. But we must refer to the sections on cancroid, as it occurs in the several regions of the body, for particular and explicit directions on this head. Amongst escharotics, a decided preference is given to the paste of *Frère Côme* as modified by M. Manec, in which burned sponge takes the place of the celebrated "burned ashes of old soles of shoes" of the original formula. The proportions are as follows:—

White arsenic, . .	1·50	grammes, about	23	grains	English.
Cinabar, ...	7·50	„ „	116	„	„
Burnt sponge, . .	3·75	„ „	58	„	„

With the addition of some drops of water, a homogeneous paste is made, which is to be applied in a layer about two millimetres thick, and then to be covered with a piece of moistened charpie. Care must be taken not to apply it over too large a surface. Our author says, he has never seen toxic effects produced after the prudent use of this caustic; though in some

cases arsenic was found in the urine after twelve hours, and continued to be detected in this secretion for eight or ten days. At the end of fifteen days, a second application must be made if the first be insufficient.

Several valuable sections remain still unnoticed, but our limits prevent us from following this laborious investigation further. We earnestly recommend the careful perusal of the work to all interested in pathological inquiry,—to the theoretical as well as the practical man. Amongst all the works which teem from the press in such abundance, we have not of late met one which bears such intrinsic evidence of an indefatigable spirit of scientific research, or one which so abounds in results. To those who have not yet familiarized themselves with the microscope as a means of clinical research, it gives the most unequivocal testimony of the value of this instrument, without whose aid the study of cancerous and cancroid diseases of the mamma, the uterus, and other regions where they occur, is as futile and incapable of being brought to anything like a condition of scientific accuracy, as the exploration of thoracic disease without the aid of the stethoscope. Neglect of either of these means of research implies a degree of professional ignorance unwarrantable in the present position of medical science, and, in our opinion, as completely disqualifies for the practice of our art in the one case as in the other.

To those about to engage in the microscopic study of cancerous and cancroid diseases, we would beg to offer a few practical observations. Before all things it will be necessary to have at least an accurate knowledge of the minute anatomy of the organ to be examined,—such as must be derived not alone from books, but from actual study of the healthy organ. Thus we must make ourselves familiar with the microscopic elements of the epidermis and chorion, before we proceed to study tumours of a cancroid nature affecting the skin. In like manner, a preliminary investigation of the mucous membrane of the uterus will be required before we come to investigate disease in this situation. Without, in fact, a practical acquaintance with the forms and dimensions of the cells to be found in the epidermis, and on the mucous surfaces in the healthy state, it will be impossible to appreciate the changes produced by disease. In the investigation of minute and delicate structures—such as epidermic or epithelial cells, cancer cells and nuclei—two subjects require particular attention in the manipulation of the instrument—viz., the use of sufficiently high powers and adequate illumination. Those who possess an English one-eighth inch object-glass have all that is practically requisite as far as

regards powers. A lens of nearly equal performance can be had from French makers at a fourth of the cost of an English one. We can especially recommend the No. 5 made by M. Nachet, of Paris. But magnifying power is not the only thing required. An equally great desideratum is proper illumination, which can only be had by adjustment of the mirror, so that the pencil of light shall fall obliquely on the object to be illuminated. In order to acquire facility in the use of the mirror for procuring oblique light, we recommend preliminary trials on some of the usual test objects—the navicula angulata, or formosa—in examining which it will be soon perceived how great a difference is produced by a slight attention to the angle of incidence of the light, in showing minute objects under precisely equal magnifying powers. For more delicate operations still, some of the prisms for producing oblique illumination now in use will be necessary. With careful adjustment of the light, an observer is surprised to perceive cells which were previously altogether invisible, or but partly made out. Measurements, as insisted on by M. Lebert, we also highly approve of[a]

Miss Martineau and her Master. By J. STEVENSON BUSHNAN, M. D., &c. London: Churchill. 1851. Post 8vo. pp. 173.

Homœopathy and the Homœopaths. By J. STEVENSON BUSHNAN, M. D., &c. London: Churchill. 1852. Post 8vo. pp. 214.

WE notice these works to call the attention of such of our readers to them as wish to obtain an able exposition of two of the prevalent and absurd delusions of the present age, which tend to interfere more or less with the advancement of medical science, chiefly from the attempts made by their votaries to have them considered as being connected with true medicine, and not with fraud and quackery. Dr. Bushnan has determined to employ his talented pen "in the exposure of the prevailing quackeries of the day in a series of publications," of which these constitute two: the former being devoted to the mesmeric, and the latter to the homœopathic, imposture. Both are most ably executed, exhibiting the careful reading and logical reasoning to be expected from the learned Editor of the Medical Times and Gazette; and we think that the profession should feel deeply grateful to him for such an employment of his time and talents.

[a] For hints on micrometric measurements, especially for estimating the magnifying power of lenses, see Dr. Lyons' Retrospect in the tenth volume of our present Series.

Leucocythemia; or, White Cell Blood in relation to the Physiology and Pathology of the Lymphatic Glandular System. By John Hughes Bennett, M. D., F. R. S. E., &c., &c. Edinburgh: Sutherland and Knox. 1852. Royal 8vo. pp. 131, with two coloured Lithographs and numerous Woodcuts.

On a former occasion, we laid before our readers a brief notice of the researches which were then being prosecuted by Professor Bennett into the pathology of the blood. In the work before us the results of his labours are thrown into a collected form, and the subject of leucocythemia may be now more conveniently studied in all its bearings, or at least in those pathological relations which the researches hitherto prosecuted have established for it. Though Professor Virchow was the first who actually applied a distinct name to this affection, decidedly the most appropriate is that adopted by Dr. Bennett, which fully expresses the pathological condition in question, namely, a state of the blood in which the white corpuscles are found in unusual abundance, and involves us in no assumption of any theory as to the nature or causation of the disease. Though many observers have assisted in collecting evidence on this singular lesion, it is to Dr. Bennett, in an especial manner, that we owe the early recognition of its importance and the fullest elucidation which it has yet received[a].

The first part of the present volume is occupied with the details of 35 cases, in 25 of which an increase in the relative proportion of the white corpuscles of the blood was proved to exist by careful microscopic examination. Of the 25 cases, 16 occurred in males and but 9 in females: the ages varied from 9 to 69. In 20 cases abdominal swelling was present; in the majority both the spleen and liver being much enlarged beyond their natural dimensions. But it is to be observed, that while in a large proportion of the cases of leuco-

[a] The chronology of the cases first observed is, as accurately as we can ascertain, as follows: Dr. Bennett's first case, March 19th, 1845, published in October, 1845, as a case of suppuration of the blood; Virchow's first case, 1845, date of first publication unknown to us, but it was described as "Leukemia;" Drs. Fuller, Walshe, and Redfern, observed cases in 1846; Dr. T. K. Chambers, in 1847; since when cases have been seen by Drs. Bennett, Quain, Parkes, Virchow, Vogel, Drummond, Wallace, Gairdner, and Heslop; and in this city by Dr. Lyons, who, in the course of last year, investigated two cases under the care of Dr. O'Ferrall in St. Vincent's Hospital; and by Dr. Neligan, who communicated a notice of a case recently under his observation to Dr. Bennett.

cythemia the spleen was enlarged, yet four cases of hypertrophy of the spleen are given, in which careful microscopic examination showed that this condition of the blood did not exist; while, again, a marked case of leucocythemia, which will be found at p. 46, exhibited no enlargement of the spleen; and in the case which follows, it is stated that "the spleen was only a very little enlarged, though soft, and abounding in corpuscles:" from all which we may conclude that there is no *necessary* connexion between the two conditions of white cell blood and enlarged spleen. We are still greatly in want of some accurate means of estimating the relative proportion of the elements seen in the field of the microscope. Professor Bennett thinks the best method of judging the relative proportions of the two orders of blood corpuscles is to observe the spaces or meshes left between the rolls or aggregations of yellow corpuscles. Even when these spaces are completely filled, the colourless globules do not amount to one-third of the coloured discs, on account of the large number of the latter which may be crowded into a very small space.

For the purpose of a more accurate estimate, we would suggest the use of an ocular micrometer, divided into squares of any convenient dimensions, with the aid of which we may ascertain, in a number of examples of normal blood, what the natural numerical proportion between the red and white corpuscles is, by counting both as they are placed under the squares in two or three opposite portions of the field, selected indifferently; the average result of a few computations of this kind, we are of opinion, would be found not far from the truth. A similar proceeding could then be had recourse to to estimate the proportion between the number of white and red corpuscles in suspected cases of leucocythemia. Until some such process be brought into use generally, it is manifest that we can learn but little from such vague statements as are usually appended to the reports of cases, "one-third as numerous," "greatly increased," &c., &c. One remarkable fact is stated by Dr. Bennett, namely, that in no case has the condition of the blood been observed to undergo any marked change after the excess of white cells in it was once discovered. "In no case has this condition been seen to appear and progress gradually, as is observed in so many other lesions."

After considering the morbid anatomy of individuals affected with leucocythemia,—the condition of the spleen and lymphatic glands,—our author next takes up the subject of the relation existing between the colourless and the coloured corpuscles of the blood, and the origin of the blood corpuscles in general.

Several ingenious observations are brought forward to show that the nucleus of the white corpuscle undergoes a series of changes by elongation and fissure, whereby it finally breaks up into two or more portions, which imbibe the peculiar colouring matter, and finally become liberated as coloured corpuscles. These, therefore, he regards, in common with Valentin and Wharton Jones, as free nuclei. After a short discussion of the function of the lymphatic glands, he arrives at the conclusion that their office is to secrete the blood. The following are the results to which he comes—results for which we by no means pledge ourselves, but which we think are highly deserving of consideration:—

"From the various facts which have been stated," says Professor Bennett, "I think we may conclude:

"1. That the blood corpuscles of vertebrate animals are originally formed in the lymphatic glandular system; and that the great majority of them, on joining the circulation, become coloured in a manner that is as yet unexplained. * * *

"2. That in mammalia, the lymphatic glandular system is composed of the spleen, thymus, thyroid, suprarenal, pituitary, pineal, and lymphatic glands.

"3. That in fishes, reptiles, and birds, the coloured blood corpuscles are nucleated cells, originating in these glands; but that in mammals they are free nuclei, sometimes derived as such from the glands, at others developed within colourless cells.

"4. That in certain hypertrophies of the lymphatic glands, their cell elements are multiplied to an unusual extent, and under such circumstances find their way into the blood, and constitute an increase in the number of its colourless cells. This is leucocythemia.

"5. That the solution of the blood corpuscles, conjoined with the effete matter derived from the secondary digestion of the tissues, which is not converted into albumen, constitutes blood fibre."

This return to the long obsolete opinions of Hewson is certainly not a little remarkable in the present advanced state of physiological science. The views put forward by Dr. Bennett require very careful consideration; and though we are not ourselves prepared to advocate them, we see that a considerable weight of argument exists in their favour. To all who are anxious to investigate the pathology of the blood, we confidently recommend this volume of Dr. Bennett's, as containing a very able and faithful account of this newly observed and highly interesting condition, so well described by his term "Leucocythemia."

A Practical Treatise on Diseases of the Skin. By J. MOORE NELIGAN, M.D., M.R.I.A., &c. Dublin: Fannin and Co. 1852. Post 8vo, pp. 439.

THERE are few more interesting classes of disease, few that have received more extensive or elaborate investigation, and yet few that have returned more meagre rewards to the inquirer, than that of the diseases of the skin. The interest in cutaneous pathology has been ever preserved, as well by the ever varying attractive character of the subject, as by the proverbial difficulties in diagnosis and therapeutics which surround its study; and the laudable desire to elucidate, and render it easy of comprehension, has from time to time induced the greatest minds to devote a thorough attention to its every detail. Notwithstanding all this,—though we can point to a host of works specially written upon this subject by eminent men of all times and nations,—yet are we compelled to confess that much remains to be accomplished ere the study may be said to be complete for the purposes of practice. The first question we naturally put on the announcement of a new book is:—Is there a necessity for it? Does it accomplish any *desideratum?* Our previous knowledge of bibliography enabling us to state what is required in the investigation of any disease, or class of diseases, compels us to affirm, that in the present case there is much to be effected in addition to the laborious efforts of such men as Rayer and Erasmus Wilson. Thus, for example, improvement in the management of cutaneous cases is evidently demanded, as the experience of every practitioner of any standing will abundantly testify. No one could have read the medical journals for the last eight or ten years without perceiving the great amount of uncertainty which shadows cutaneous therapeutics. There seems, in fact, to be no general principle or guide on which the practitioner could rely. Whenever any case did not yield to the "usual remedies" (which, by the by, are themselves multiform), recourse must be blindly had to a series of trials with alteratives of every hue and strength. No wonder that the ranks of cutaneous empirics have enlarged in the midst of such confusion amongst the regular forces. Nevertheless, such has ever been the case in the progress of medical science generally; and it seems necessary to the evolution of a true line of action that a period of vacillating trials and uncertain experiment should be a preliminary ordeal. It may be that little permanently useful could otherwise be tested or obtained; and hence we must regard these seasons of doubt, albeit *opprobria medicorum*, as but "necessary evils."

The diagnosis, too, of cutaneous diseases has ever been a stumbling-block to progress in this department. Not merely is this difficulty felt by the student or the junior practitioner, but it is freely acknowledged by lecturers and authors themselves; and were it requisite, we could adduce many instances of the grossest discrepancies amongst the most distinguished dermatologists. Doubtless, much has been done recently in our works on practice of medicine, and more especially in monographs on cutaneous maladies, to throw fresh light on this knotty point, and we believe with a certain degree of success. Willan assuredly first pointed out the way, and his followers have now trodden it into an established highway: still it is beset with by-roads and diverticula, which frequently confuse the traveller, and occasionally put him off the right path. There is necessity, then, for fencing the original way so securely and adequately as to prevent mistakes in future.

Again: the present most advanced pathology of the skin is open to many serious objections. It certainly teaches us the morbid anatomy of any cutaneous disease, but is egregiously wanting in that more important, essential, and practical part, which informs us of the intimate causes of the pathological changes, and their connexion with natural or diseased states of the system or lesions of other organs. General cutaneous pathology is deficient; yet the state of this study in regard to some other organs manifests a sufficiency of material for the required purpose. Such being the position of our knowledge of cutaneous diseases, it is plain that a clear necessity existed for a new volume on this subject. It is now our duty to see whether the objects required have been in any degree accomplished or facilitated in the work before us.

From the Preface we learn that Dr. Neligan "has been influenced by a desire to offer, as an aid to the diagnosis and treatment of an important class of diseases, the results of an experience acquired during several years' special attention to the study of cutaneous eruptions." On the subjects of diagnosis and treatment, then, we are led to expect some original observations, which may attempt to improve the decidedly imperfect knowledge we at present possess. Pathology is not mentioned here, though there are allusions in every chapter to the results of other inquirers on this head. If this volume, then, yields an advance in cutaneous study, it must be in diagnosis or treatment.

The author opens his subject by a chapter on cutaneous nosology. He is in favour of the *artificial* system of Willan, which certainly has its advantages over a more natural one. At the same time, if practicable, why not combine the two?

why not have the main divisions natural—that is, pathological or anatomical, and the minor, arranged according to leading visual charaters? There can be no doubt that the latter "is especially one more easy to be remembered; it is one which aids us considerably at the bed-side, for it requires a less complex process of reasoning than a natural classification to discover by its agency what may be the disease in any special case."—p. 13. No doubt, and were this the whole object to be gained, nothing would be more desirable, but, unfortunately, the reasoning faculty must be just as severely taxed as ever, after you have discovered the name, in order to divine the nature or true pathology of the case. We do not, in making these remarks, desire to conceal the fact, that in the present state of cutaneous pathology it is most difficult, if not impossible, to construct a natural classification free from defects. But it is one at which all our study of cutaneous diseases should aim, with the view of ultimate attainment.

In the classification which Dr. Neligan adopts he does not wish to imply that there is any originality, his "chief object being to endeavour to simplify a subject which has often not received from the student and practitioner the attention it merits, owing to the difficulties with which complicated arrangements and ever-changing nomenclature have invested it;" and he further takes occasion, while generally approving of the artificial arrangement, to refer to several weighty objections which pertain to it,—objections which, we agree with him, "only prolonged practical experience can nullify."

Dr. Neligan divides cutaneous diseases into ten groups or orders, as follow:—

1. EXANTHEMATA.
2. VESICULÆ.
3. PUSTULÆ.
4. PAPULÆ.
5. SQUAMÆ.
6. HYPERTROPHIÆ.
7. HEMORRHAGIÆ.
8. MACULÆ.
9. CANCRODES.
10. DERMATOPHYTÆ.

Adding two supplementary groups, SYPHILIDES and DISEASES OF THE APPENDAGES OF THE SKIN.

Each chapter is occupied with the consideration of one group of cutaneous diseases. First are pointed out the leading characters common to all the divisions; next a separate description of each division, with a notice of its varieties, is given; then follow the essential characteristics or the diagnosis, the prognosis, and finally the treatment.

Comparing Dr. Neligan's account of each group with the descriptions of other authors, we must say he bears off the palm for clearness, conciseness, and rigid plainness of expres-

sion. This style enables him to compress much in a single sentence without in any degree injuring the sense, but, on the contrary, making it more comprehensible and impressive. His simplification of the divisions is a striking feature in his descriptions, and yet, by introducing the accumulated subdivisions of other authors as varieties merely, we lose nothing of any real importance. His diagnosis is as it should be, clear and definite, and the broad lines of demarcation are prominently distinguished, so that, rising from even his shortest descriptions, you do not feel at all the want of plates or illustrations, the very severest test that can be given to try the effectiveness of cutaneous definitions.

The pathology of any of the cutaneous groups is but slightly touched upon. As this was, however, not a special object of the work, we are not to expect any elaborate particulars. Sufficient, however, is given to inform the reader both of the seat and structure engaged, and the constitutional condition present. Indeed his opening descriptions of each group may be deemed pathological definitions. It is, however, upon the most practical and important branch of his subject that Dr. Neligan lays claim for originality and improvement, viz.: therapeutics. By far the largest proportion of the volume is devoted to therapeutic considerations. Not merely are full details of treatment and formulæ given at the close of each section, but an entire chapter is devoted to "those general points in therapeutics which are specially applicable to this class of affections." In this chapter he reviews *seriatim* the remedies most usually employed and the manner of their administration, and intersperses a few novel formulæ of at least a suggestive use to the practitioner.

In the space allotted to us it is impossible to give this section the notice its many excellencies demand; we must, therefore, content ourselves with stating a few of the general principles which seem to guide our author in his treatment, and refer to his management of some obstinate forms, and to some of the new formulæ with which the volume abounds.

As far as an attentive scrutiny can enable us to judge, we think that Dr. Neligan's main lines of treatment are the only correct guides. He considers in every case the local condition of the eruption, whether that of irritation or positive inflammation, or one of debility, or one of habit; secondly, he looks into the constitutional state, whether plethoric, anemic, scrofulous, &c.; and thirdly, he examines his patients to see what condition co-exists in other organs which may influence the local eruption, or which the treatment otherwise indicated may effect. Thus armed and fortified, he uses the weapons of the

Pharmacopœia with the greatest rational prospect of success. In carrying out his object thus arrived at, he of course uses the ordinary remedies indicated; but in the more intractable affections, a considerable change from the ordinary practice may be readily observed. This we shall make manifest in a few sentences.

Thus when ringworm (that is, according to Dr. Neligan, *herpes capitis*) becomes chronic, constitutional treatment is required, such as preparations of iodine, especially the green iodide of mercury, with or without tonics; and among local applications he prefers the brown citrine ointment or one of sulphate of iron, white wax, and glycerine, to be applied twice a day, after sponging with a carbonate of potash lotion. In *chronic impetigo of the scalp*, Dr. Neligan advises close cutting of the hair, removal of the crusts by poultices and alkaline washes, afterwards the soda ointment and tepid milk lotion, and then the internal exhibition of the green iodide of mercury, with hydrargyrum cum creta and dried carbonate of soda, in mild but alterative doses. When strictly enforced, "this plan of treatment," he says, "seldom fails to cure the disease, even in the most chronic forms, in from six weeks to three months."

Chronic prurigo has been always deemed most inveterate, especially when it attacks the elderly. Dr. Neligan's plan of management is to attempt, first, to improve the general health, by altering the secretions, if vitiated, by tonics—especially the nux vomica,—by purgatives, &c. &c.; then, viewing the disease more as an example of hyperæsthesia, he enjoins the internal administration of sedatives, such as aconite, Dover's powder, and conium, and locally the hot sulphuro-alkaline bath; the opiate, hydrocyanic acid, lead, and mercurial washes; and chloroform and iodide of lead ointments, with the addition of glycerine. These examples will suffice to show that Dr. Neligan's plan of treatment is not merely in our opinion calculated to be effective, but presents the freshness of originality, which, in the case of cutaneous diseases, was much to be desired.

Our author's opinion of several remedies new and old are given in the chapter on Therapeutics; from which we learn that *collodion* has not preserved the reputation with which it was originally introduced as a local application in cutaneous diseases. Bandages are useful in all cases where the skin has become relaxed or likely to lose its elasticity. Baths are not so generally lauded as formerly: they are, however, still useful as cleansing, antiphlogistic, and stimulant agents. Formulæ are given for the preparation of gelatine, alkaline, iodine, sul-

phur, and mercurial baths. Emollient *cataplasms* are recommended in all crusted cases, and as antiphlogistics.

Caustics, whether of nitrate of silver, iodine, or chloride of zinc, are useful to destroy morbid growths; but their effects in chronic eruptions are at best doubtful. Dr. Neligan recommends the addition of glycerine to *lotions*, as it aids in keeping the part in a constantly moist state. Formulæ are appended for stimulant alkaline, sedative alkaline, glycerine, astringent, sulphurous, and strychnine lotions.

Ointments sometimes disagree, especially in most forms of eczema: our author, however, invariably uses and recommends the basis of any ointment prescribed to be either cold cream (ceratum Galeni), or the French cucumber ointment, according to the prescription of Henry and Guibourt, which has peculiarly calmative and healing properties. Formulæ are annexed for the chloroform, camphor and hemlock, and several astringent ointments.

Dusting powders, as flour, lapis calaminaris, krameria and chalk, starch, &c., are useful in erysipelas and discharging eruptions.

Soaps are not in general useful, except in chronic and non-inflammatory diseases, without breach of surface. He has not found the medicated soaps of Sir Henry Marsh at all useful, but acknowledges his trials of them have been but few.

Several novel formulæ for medicines administered internally are given at the close, including preparations of the bromides of mercury and arsenic, the Indian sarsaparilla, &c. Some judicious but brief remarks upon diet and other hygienic measures to be adopted in cutaneous diseases bring the volume to a close.

From the above imperfect sketch of this volume, and our remarks thereon, our readers will be enabled to see its bearing and scope. Dr. Neligan has evidently opened up new ground in therapeutics, which, indeed, we should have expected from one who has made the Pharmacopœia a chief study, and who has been already before the profession as an author on cutaneous diseases. The present work forms a favourable contrast to the voluminous and disputed details of many of his predecessors, and will, we feel assured, be admirably conducive to facilitating the study of the student, and improving the practice of the practitioner.

We are gratified to find that Dr. Neligan purposes shortly issuing a series of plates in illustration. Though always an attractive and agreeable auxiliary, in the present instance the want of drawings incorporated with the work is but little felt.

PART III.

MEDICAL MISCELLANY.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

TWELFTH SESSION.—1851–52.

Malignant Tumour in the Orbit.—Professor R. W. Smith exhibited an eyeball which he had lately extirpated. The case was that of a man, aged 51, who had been admitted into the Richmond Hospital upon the 8th of September, 1850. He stated that eight years ago he was suddenly attacked at midnight with severe pain in the eye, and that a violent inflammation followed, which ended in total loss of vision; for this attack he had been salivated. Ever since, upon exposure to cold or wet, the eye became inflamed and swollen, and paroxysms of pain occurred, shooting through the temple, and from the orbit backwards to the brain. Ten weeks before admission, he had an unusually severe and prolonged attack of this description, for which he was again salivated. Three weeks before he applied at the hospital, and while his mouth was still sore, he went out to work in his farm, and upon the following day suffered a great increase of pain; and the eye was attacked with swelling, which soon extended to the eyelids, and continued to augment up to the time of his admission into the hospital, when the parts were in the following state:—The eyeball, seemingly greatly enlarged, projected from the orbit, the upper lid, of a bluish red colour, being stretched over its superior and anterior portions; the lower lid was œdematous and everted, exposing the conjunctiva, which was in a state of chemosis, exceedingly vascular and covered with superficial ulcerations. The conjunctiva of the upper eyelid was in a similar condition, and that covering the eye was so much swollen as nearly to conceal the cornea, which occupied the lower and inner part of the tumour formed by the enlarged eyeball, and was a little opaque, but not so much so, as to obscure the lens, which was seen to be perfectly opaque and white. The surface of the tumour near the supra-orbital ridge was nodulated, and pressure here produced pain of a severe charac-

ter and shooting back towards the brain. His general health was unimpaired.

The disease being obviously of a malignant nature, it was resolved in consultation to extirpate the eyeball; the operation was performed upon the 20th September; the entire contents of the orbit were perfectly removed, nothing being left but the periosteum. Upon dividing the optic nerve, a profuse hemorrhage ensued, in arresting which some difficulty was experienced; the cavity was dressed from the bottom. Upon the 29th, a singular train of nervous symptoms set in. The right side of the forehead appeared to droop, and its wrinkles were obliterated; there was numbness above the right brow, and inability to open the mouth, which was drawn a little to the left side; the right side of the face was smoother than the left, and appeared swollen; the alæ nasi were somewhat dilated, his speech was thick, and he could not whistle; there was, in short, a mixture of the symptoms of paralysis of one side of the face with those of trismus: they gradually, however, subsided, and had all disappeared by the 10th of October. He left the hospital upon the 26th, the wound being perfectly healed. In less than three months, however, the disease re-appeared in the orbit, and the man died in the country six months after the performance of the operation.

Upon examining the parts which had been removed by the operation, the eyeball was found completely atrophied, and the lens opaque, hard, and shrivelled; the optic nerve was also atrophied, but was free from any malignant disease. The malignant growth seemed to have originated in the orbit behind the globe of the eye, which it had pushed forwards, and the two posterior thirds of which it had surrounded; it was soft in its texture, uneven upon its surface, and its interior presented the characters of an encephaloid tumour, and contained a large proportion of melanotic matter.

Tumours of this description, originating, not in the globe of the eye, but in the orbit, were not, Professor Smith remarked, of very frequent occurrence. Mr. Travers has, however, given a description of such cases; he says:—"An extraordinary globular tumour is formed around the ball, of which the perished cornea forms the centre. It projects, stretching and so separating the lids, that they gird tightly the base of the enormous swelling. I have seen children subjects of this affection. The growth is sometimes confined to the upper or frontal aspect of the orbit. The upper lid is then prolonged and stretched over the globe so tightly, that it is difficult, if practicable, to obtain a view of the latter. The medullary matter is of a granular, ricy consistence, and pervades and destroys the muscles, periosteum, and finally, the bony vaults of the orbit. I have seen its extirpation boldly performed; but its re-appearance has been almost immediate, and its progress quick to destruction"[a]. Cruveilhier has described similar cases under the name of melanic cancer[b].—*December* 20, 1851.

[a] Medico-Chirurgical Transactions, vol. xv. p. 238.
[b] Anatomie Pathologique, liv. xxxix. planche v.

Lymph from the Bronchial Tubes.—Dr. Banks exhibited a specimen of "bronchial polypus," which he had recently received from Dr. Kelly of Mullingar, and gave the following history of the case. A boy, aged 12, was first seen by Dr. Kelly on the 24th of December, 1846, having then been four days ill. After exposure to wet and cold during the day, he was seized in the evening with severe rigors, succeeded by headach and other febrile symptoms. Upon the following morning he complained of pain shooting through the right side of the chest, together with cough and scanty viscid expectoration; his pulse was 110; he had intense dyspnœa, paroxysmal cough, and lancinating pain beneath the right nipple; there was dulness on percussion over the greater part of the right side of the chest inferiorly; the left side gave no indication of disease. Upon the 26th of December, after a paroxysm of coughing of unusual severity, and attended with more than ordinary dyspnœa, he spat up a large mass of plastic lymph, more than four inches in length, and presenting an exact cast of the right bronchus, with numerous ramifications. The dyspnœa and all the urgent symptoms at once ceased; the right side became more resonant on percussion, and a crepitus was audible over a considerable extent; he improved daily, and on the 30th was perfectly convalescent.

On the 26th of February, 1851, Dr. Kelly was again called to see him; he found him labouring under fever of the prevailing type; on the third day from the accession of the fever he was again seized with symptoms in all respects resembling those of his former illness. On the 2nd of March, after a long continued fit of coughing, he expectorated another large mass of false membrane; and on the 5th, after another paroxysm of coughing and difficulty of breathing, so great as to lead his father to believe that the boy could not survive it, he expectorated a third mass of great size, and similar to the others. He soon recovered, and has since remained in the enjoyment of perfect health. Dr. Kelly reports, that on the 26th of January last he examined this boy's chest, and found that the right side measured an inch less than the left; and that inferiorly it was not so resonant as the opposite side.

This case, Dr. Banks observed, differed from those which he had brought before the Society upon former occasions, in its acuteness, and in its coincidence with pneumonia or pleuro-pneumonia, a complication which had been found to exist in some examples of the disease occurring in the course of typhoid fever, and also during the influenza of 1837 in Paris. It does not seem that there is any unequivocal sign that can aid us in forming a correct diagnosis in these cases, before the expulsion of some of the false membrane, unless the phenomenon noticed by Barth and Caseaux should happen to be present, namely, the "*bruit de soupape*," and that this should be looked upon as a diagnostic mark worthy of confidence. Dr. Banks remarked in conclusion, that the treatment adopted by Dr. Kelly in this case, namely, the use of calomel and tartar emetic, was that upon which most reliance was to be placed.—*February 7th*, 1852.

Cancer of the Stomach and Peritoneum.—Dr. Lees exhibited a specimen of cancer of the stomach and peritoneum, taken from the body of a man aged 50, who died in the Meath Hospital. He had been admitted on the 6th of February, complaining of pain in the stomach whenever he took solid food, which was rejected in about a quarter of an hour after it was swallowed, but fluids were rejected at once. His complexion was pale and sallow, and he was greatly emaciated. He also suffered from constipation and tympanitic distention of the abdomen; the urine deposited large crystals of lithic acid; the abdomen was not tender on pressure, nor was there any dilatation of the superficial veins.

The patient stated that he had enjoyed tolerably good health until five weeks ago, when he vomited up a substance like coffee-grounds; the vomiting recurred several times daily for a fortnight, after which it only came on after eating; he had no dysphagia. Upon examining the abdomen, a hard, irregular, and fixed tumour could be felt in the epigastric region; it did not pulsate; several smaller tumours could be distinguished near it; he had constant thirst and desire for cold drinks. The matter rejected from the stomach, when examined under the microscope, was found to contain nucleated cells, similar to those considered characteristic of cancer. The man gradually sank, and died three weeks after his admission.

Autopsy.—The abdomen contained about two quarts of serum, and the greater part of the peritoneum was converted into a mass of gelatiniform cancer, more especially those processes of it which invest the diaphragm and form the ligaments of the liver. The great omentum was similarly changed, and contracted into a hard, cancerous mass, which was in contact with the cardiac end of the stomach. This organ was also reduced in size, and the entire of its pyloric half contained a large growth of areolar cancer, uncovered by mucous membrane. Dr. Lees remarked that the chief points of interest in the case were the short duration of the symptoms, the rapid rejection of what was swallowed, although the cardiac half of the stomach was free from disease, the absence of pain or tenderness on pressure with such extensive disease of the peritoneum.—*February*, 28, 1852.

Tubercular Ulceration of the Larynx and Trachea, &c.—Dr. Banks exhibited a series of specimens of tubercular disease of the air-passages, the mesenteric glands and the intestines, and gave the following history of the case. E. C., aged 31, a discharged soldier, was admitted into the Whitworth Hospital, February 10, 1852, in a state of extreme debility and emaciation; his respiration was difficult, he had laryngeal cough and frothy purulent expectoration; his pulse was 108; he complained of acute pain in the region of the larynx and trachea, increased by pressure, coughing, or speaking; he had also difficulty in swallowing solids. He stated that, though of intemperate habits, he had never (with the exception of an attack of jaundice) suffered from ill health, until eight months ago, when, while

quartered at Halifax in America, he got cold, and while he was suffering from cough and expectoration, his regiment was ordered home. During the voyage he was much exposed to cold and wet, and upon his arrival in England, he was admitted into the hospital at Fort Pitt, where he remained for two months, gradually getting worse, and losing flesh and strength; the expectoration continued, and he became subject to violent paroxysms of cough; he became hoarse, his voice gradually altering and getting weak, so much so as to be nearly inaudible when he was admitted into the Whitworth Hospital, at which period he also had diarrhœa.

After a careful examination of the chest, the only observation made was, that the respiratory murmur was somewhat feeble; there was no indication of the presence of mucus in the tubes; all parts of the chest were resonant under percussion; the epiglottis seemed healthy, and there was nothing abnormal in the appearance of the fauces.

Upon the 19th February the diarrhœa ceased, but the cough and pain in the trachea had become more severe, the respiration was hurried and stridulous, and in the act of deglutition fluids occasionally returned through the nostrils. There was now a slight comparative dulness detected at the inferior angle of the right scapula, and a slight subcrepitant râle; the vesicular murmur was feebler beneath the clavicles than elsewhere, but there was no diminution of the resonance on percussion. On the 23rd he complained of pleuritic pains in the right side, and died upon the following day.

Autopsy.—Upon opening the chest there was no collapse of the lungs; they were both emphysematous over their entire surface, and at their apices large transparent masses of emphysema existed; both lungs contained tubercles and tubercular cavities; there were traces of recent pleuritis upon the right side; a very large, irregular ulcer existed at the base of the epiglottis, and the trachea, down to its bifurcation, presented a series of ulcers of various sizes and depths, some of which had denuded the cartilaginous rings of the tube. Numerous irregular excavated ulcers existed in the small intestines, and the mesenteric glands contained scrofulous matter.

Dr. Banks, in commenting upon the preceding cases, alluded to the difficulty which attends the diagnosis of pulmonary phthisis at an early stage, when complicated with laryngeal disease, and quoted the observations of Dr. Stokes upon such cases, who has remarked that, in the obstruction of the larynx, there is nothing to interfere with the use of percussion, a statement which is, without doubt, perfectly correct; in the present instance, however, the emphysematous state of the lungs rendered the results of percussion fruitless. The state of the larynx, however, the pain along the course of the trachea, the absence of any syphilitic taint, and of any evidence of tumour pressing upon the windpipe, together with the general symptoms, rendered it comparatively easy to infer the existence of tubercular disease of the lungs in this case.—*February* 28, 1852.

Ulceration and Stricture of the small Intestine.—Dr. Mayne laid before the Society a remarkable specimen of extensive ulcerations and contractions of different portions of the small intestine, of which he gave the following account:

The subject of the disease was a man, aged 37, who, four years ago, was seized with what he supposed to have been inflammation of the bowels, but Dr. Mayne was unable to ascertain the exact nature of the attack.

After recovering from this acute disease, he began to suffer from various dyspeptic symptoms, which never afterwards completely disappeared; the least irregularity in diet was certain to produce either gastrodynia or acidity of stomach, or flatulent colic, ending in diarrhœa; vegetables invariably disagreed with him.

In September, 1850, he was suddenly attacked at night with symptoms of severe gastro-intestinal irritation; he had obstinate vomiting, followed by profuse purging. From this period he scarcely enjoyed good health for a single day; constantly recurring attacks of very painful colic constituted the chief source of his sufferings. At first these fits used to commence at a certain period after meals, and were then also mitigated by a spare regimen, and an occasional laxative, but latterly they have attacked him several times in the course of twenty-four hours, and defied all remedies.

The pain in general commenced at the umbilicus, and was immediately followed by the formation of a large, undefined swelling, which was apparent to the eye, and always exhibited a singular heaving or vermicular motion; it was attended with grinding pain, amounting to absolute torture. During these paroxysms of distention and pain, the swollen portion of the abdomen yielded on pressure a gurgling sound. The curious heaving movements were evidently the peristaltic motions of an enormously hypertrophied intestine, seen through the attenuated abdominal parietes.

By degrees his constitution began to give way from the severity of his sufferings; his appetite failed; emaciation proceeded rapidly, profuse diarrhœa set in, and he became a prey to the most extreme despondency. At length, on the 24th of February, in the evening, he suddenly exclaimed that "something had gone amiss with him," and that his death was at hand. A piercing pain had seized him in the right iliac region, quite unlike any of his previous sufferings; this was soon followed by still more alarming indications; his breathing became hurried, his countenance assumed an anxious expression, his features were drawn, his surface covered with a cold sweat, his voice became husky, the abdomen tympanitic and exquisitely tender to the touch, vomiting and hiccough set in, and he died in about thirty-six hours from the accession of these new phenomena.

Two of this man's sisters had died of pulmonary consumption; and he had himself, when aged about 17, suffered for several months from chest symptoms; he subsequently, however, completely recovered, and enjoyed the most robust health until four years before his death. His occupation was that of a land steward.

Considerable difference of opinion having been expressed by his physicians as to the exact nature of the disease of which he had died; his friends requested that a post-mortem examination should be made, which was accordingly done by Dr. Mayne, assisted by Dr. Wyse, whose patient he had been for several months.

Upon laying open the abdomen, a quantity of gas escaped, and the anatomical characters of recent and acute peritonitis presented themselves, particularly in the iliac fossæ, and in the pelvis, where sero-purulent fluid was collected. A small perforation was discovered in the ilium intestine, about two feet from the cœcum; the opening was not larger than a grain of snipe shot; through it, however, the fluid contents of the intestine readily escaped.

On further examination, the small intestine was found to exhibit internally four very extensive ulcers, placed six or eight inches from one another; they had each extended in the form of a zone or ring completely around the gut, destroying the mucous membrane totally, and the muscular coat partially. The lowest of them was situated about one foot and a half from the cœcum; they varied in breadth considerably, the narrowest being about one inch across, the widest two inches and a half. It was the second of these ulcers (counting from the cœcum) which had perforated the intestine; the mucous membrane between them presented evidences of having been the seat of chronic inflammation.

Two important consequences had resulted from these ulcers, viz.:—1st. Great contraction of the tube of the intestine at the seat of each of them, and enormous dilatation of the bowel above the first or highest. 2nd. Perforation of the intestine, and fatal peritonitis. A full-sized catheter passed through the constricted portions, while (in point of dimensions) the dilated part resembled the stomach much more nearly than a small intestine.

The coats of this dilated portion were enormously hypertrophied; its muscular tunic was thicker than that of the rectum; its interior contained a large quantity of gas, some excrementitious fluid, and foreign bodies in great number and of various descriptions. They were found to consist of bones, and portions of bones, orange pippins, pieces of horn, bark of wood, pieces of wood and grape-seeds. Some of them were very rough, and even spiculated, and their irregular surfaces, by irritating the ulcerated bowel, must have largely contributed to produce the severer paroxysms of pain which the patient so often suffered from.

The dilated portion of the intestine, at its widest part, was eight inches in diameter.

The mesenteric glands corresponding to the ulcers were greatly enlarged, and contained scrofulous matter in great abundance.

The mucous membrane, throughout the remaining portions of the small intestine, was more vascular than natural, and all the tunics were more or less thickened. The canal, from the lowest of the ulcers to the anus, was nearly empty, and very much reduced in caliber; the cœcum, the colon, and the rectum, respectively,

scarcely equalling the ordinary dimensions of the small intestine. The remaining abdominal viscera were healthy.

There was a large deposit of crude tubercle, without any appearance of softening, in the apex of the right lung.

Dr. Mayne was of opinion that, in this case, the ulcer of the intestines should be considered as being of a tubercular nature. They had evidently originated in Peyer's glands, and from thence had gradually crept around the intestine. That they were of a tubercular character was rendered extremely probable by the man's constitutional predisposition, as well as by the co-existence of scrofulous disease in the mesenteric glands, and tubercular deposits in the right lung.—*February* 28, 1852.

Dislocations of the Patella.—Professor R. W. Smith exhibited two casts,—one showing a complete, and the other a partial luxation of the patella outwards. In the case of complete luxation, the accident had happened when the individual was about twelve years old; in leaping across a ditch, he fell and struck the front of the knee against a stone. Two days afterwards he was seen by two surgeons, who (probably in consequence of the great amount of effusion which had taken place) did not recognise the injury, and the bone remained ever after displaced. Violent inflammation ensued, and an abscess formed in the thigh above the knee. Six months elapsed before he was able to use the limb. The cast was taken twenty-two years after the period of the occurrence of the accident, and showed the patella lying upon the external surface of the outer condyle of the femur, with its anterior surface directed outwards, and its posterior inwards. The surface of the femoral condyles could be plainly felt, and the hollow between them became very striking when the knee-joint was flexed. The patella was perfectly moveable, and there was no change of form in any of the bones composing the articulation. The motions of flexion and extension were unimpaired, and the man enjoyed, in every respect, perfect use of the limb.

In the other case, the patient was a young woman, and the displacement occurred from muscular contraction, during an epileptic convulsion; the patella was drawn outwards in such a manner that the inner third of its articular surface lay upon the front of the outer condyle of the femur, while the remaining two-thirds projected beyond the condyle, forming a very striking and remarkable prominence at the outer side of the joint; this portion of the cartilaginous surface of the bone could be easily felt through the integuments. The patella rested in an oblique position, its anterior surface being directed inwards, and its internal border turned backwards; the surface of the inner condyle could be plainly distinguished. A very slight increase of muscular force would apparently have been sufficient to have turned the bone completely upon its inner edge, a form of displacement which is, probably, always preceded by a partial luxation outwards, and which is favoured by the form of the outer condyle of the femur; for during the act of

displacement outwards, the patella, so to speak, is ascending the inclined plane which the upper part of the condyle represents, so that during the motion outwards, its anterior surface is directed inwards, and its inner edge backwards; when it has reached a certain point, it will then chiefly depend upon the amount of muscular force whether this partial displacement outwards shall continue, or that formidable injury, luxation of the patella upon its edge, be produced.

The complete luxation seems in general to result from external violence, while muscular action is sufficient to produce the partial displacement ; in the former the posterior surface of the bone is turned inwards, but in the latter it is directed outwards.—*February* 28, 1852.

Adhesion of the Pericardium; Pneumothorax.—Dr. M'Dowel exhibited the thoracic viscera of a boy, aged 19, who died lately in the Whitworth Hospital. He had been in the hospital for three months, during which period the case presented all the usual phenomena of pneumothorax, with effusion. The perforation of the lung had taken place without any marked symptoms to denote the time of its occurrence. Death occurred rather suddenly.

Upon examination after death, the fistulous aperture (which was in the left lung) was found to be situated lower down than usual; it occupied the posterior surface of the lung, at the junction of the middle and inferior thirds. The lung itself was not so much diminished in size as it generally is in such cases, probably in consequence of the very great amount of tubercle deposited in its substance. The superior half of the right lung was also filled with miliary tubercles.

The pericardium had formerly been the seat of inflammation, and was almost universally adherent to the heart; the dense and intimate nature of these adhesions showed that a considerable time had elapsed since pericarditis had existed. In the few spaces left by the non-adhesion of the two layers of the serous membrane, a few ounces of clear, amber-coloured serum had collected.

Dr. M'Dowel remarked, that it was interesting to observe the effects of this morbid change upon the heart itself. Upon former occasions preparations had been exhibited to the Society, showing atrophy of the muscular structure of the heart, consequent upon, or at all events co-existing, with an obliterated pericardium, but in this case the reverse was found, the heart being hypertrophied, the enlargement being the more remarkable when it is remembered that in the bodies of those who die of chronic phthisis the heart is so generally found to be smaller than natural. There was in this instance no valvular disease.

The specimen corroborated the opinion expressed by Mr. Adams in his memoir upon diseases of the heart, in the fourth volume of the Dublin Hospital Reports. Mr. Adams observes: "Although we not unusually find the heart enlarged when the pericardium is adherent to it, this has not been, as far as I know, referred to as a cause of its inordinate growth; yet, when we reflect that in the na-

tural state the heart has no vascular connexion with the surrounding organs, and is only supplied with two small arteries, we can readily conceive what a new impulse its nutrition must derive from the immense number of vessels which from the adherent pericardium will pass directly into the muscular substance of the heart."

That a condition the reverse of hypertrophy might not be induced after adhesion of the pericardium had existed for a great number of years, Dr. M'Dowel said he was not prepared to deny; but he was of opinion, that if any alteration took place in the heart soon after the obliteration of the pericardium, that change was most likely to consist in an increase of thickness in the muscular structure of the organ.—*March* 6, 1852.

Aneurism of the Abdominal Aorta.—Dr. M'Dowel presented two specimens of this disease, and made the following statement:—

Case I.—James Kennedy, aged 30, a healthy, robust-looking man, was admitted into the Whitworth Hospital, on Thursday, February 25. He sought relief for a pain in the abdomen, from which he had suffered very severely for five weeks previously. This pain was chiefly situated in the epigastric and hypochondriac regions, and thence radiated over the greater part of the abdomen. He complained of but little else. His bowels, however, had latterly been much constipated, and he had frequently had much flatulency of the stomach, but no nausea or vomiting. On the following day (Friday) he was first visited. His abdomen was examined with the hand; the epigastrium was intolerant of pressure, and the liver could be felt projecting below the ribs, enlarged and tender to the touch.

No other tumour could be detected. It is to be regretted that no stethoscopic examination was made. Leeches, with blue pill and rhubarb, were prescribed. After being leeched he felt better, and suffered much less pain; but in the evening, about seven o'clock, he suddenly expired.

It was subsequently ascertained that he had been under treatment for several weeks before he applied at hospital, but that nothing which had been prescribed had afforded him any relief; and also, that in June last he had suffered from a precisely similar attack, which only lasted, however, for one week; and from that time until five weeks before admission, a period of more than six months, he had remained perfectly well. This is a singular feature in the history of the case.

Post-Mortem Examination.—On opening the abdomen the peritoneal cavity was found to be filled with a great quantity of blood, which had entered through a rent in the ascending layer of the transverse mesocolon; behind this lay an aneurismal tumour, as large as a lemon, which sprang from the front of the aorta, just at the origin of the cœliac axis, a portion of this vessel which is peculiarly liable to aneurism. The sac was compressed laterally by the crura of the diaphragm, and lay behind the gastro-hepatic omentum above the stomach, and below and behind the liver. It had given

way anteriorly; a large portion of the coagulum had been driven out of the sac, and lay loose in the sac of the peritoneum.

Observations.—It may be said that pain of a peculiar kind, viz., of a neuralgic nature, was in this case the only indication of the disease. This illustrates the value which attaches to this symptom, and its importance as a diagnostic symptom of abdominal aneurism, —a fact first prominently put forward by Dr. Law, and often dwelt on by other members of this Society. In this case the pain did not depend on caries or erosion of the vertebræ, for the tumour was not even connected with the spine, but on the pressure which the tumour exercised on the nerves of the solar plexus which were stretched over its anterior surface, and which are seen in the dissection to be unusually large and distinct.

Dr. M'Dowel observed that in practice three varieties of cases of abdominal aneurism were met with. In one class of cases the disease manifests itself by a distinct and pulsating tumour, accompanied by various well-known general symptoms. In all such cases the disease is obvious. Forming a second group instances have been recorded in which an abdominal aneurism gave rise to no symptoms whatsoever, and of which the specimen now presented is an example.

Case II.—A middle-aged man, named ———, was admitted into the Richmond Hospital, several years ago, under the care of Dr. Hutton, labouring under laryngitis, which ultimately proved fatal. He never at any time had suffered from symptoms referrible to aneurism, yet, on the examination after death, this aneurismal tumour was found. (Specimen exhibited.) It is of considerable size, and springs from the front of the aorta. It is extremely interesting to observe, that the disease in this case was undergoing a spontaneous cure, for we find the sac nearly completely filled with firm laminated coagula, the outermost of which adhere to the inner side of the sac, so that the current of the blood was once more confined nearly within its original limits. Intermediate between these two classes is a third, of which the case first detailed is an illustration, in which a few symptoms exist (perhaps only one), to lead us to suspect the existence of aneurism. In such cases it is by a recognition of physical signs that we are enabled to detect the disease even in a very early stage. Dr. Corrigan has shown that at this period the aneurismal *bruit* may be audible only when the patient is placed horizontally; as, according to his view, the tension of the sac, from the weight of the column of blood in the aorta, prevents the production of the *bruit* in the erect position. Hence the necessity of careful examination with the stethoscope in cases of suspected aneurism, not only along the course of the aorta, both before and behind, but also in different positions of the body, as well in the horizontal as in the erect position.—*March* 6, 1852.

Congenital Luxation backwards, with Chronic Rheumatic Arthritis of each Shoulder-joint; Chronic Rheumatic Arthritis of the Hip-joint.—Professor R. W. Smith exhibited the shoulder-joints of a man aged

60, each of which presented an example of congenital luxation of the head of the humerus backwards upon the dorsum of the scapula, and in whom, at an advanced period of his life, the malformed joints had each become the seat of chronic and rheumatic arthritis, a well-marked example of which disease was likewise found in the right hip-joint of the same individual. The communication is here merely placed upon record, as the details of the dissection will be published in full upon a future occasion.—*April* 3, 1852.

On the Application of Gutta Percha in Small-Pox. Note by Dr. Graves.

(*See page 7 of our present Number.*)

It is of great importance, and essential to the success of this treatment, to observe, that the *gutta percha* solution should not be applied to the face until the pustules are fully maturated, or even begin to exhibit the first appearance of collapse, as indicated by the well-known central depression on the apex: applied then, the solution is of the greatest service; *applied before maturation, it is mischievous.*

A Case of Primary Amputation at the Shoulder, consequent on a Gun-shot Wound of the Arm close to the Joint, occurring in a Coloured Man. By E. T. W. Mandeville, Assistant Surgeon, 66th Regiment of Infantry.

Dommon, a Kaffir, aged 19, was wounded near Fort Beaufort, in Southern Africa, by the Fingo cattle-guard, in the beginning of the present Kaffir war, on the 13th January, 1851. He was obliged to walk nearly two miles after the receipt of the injury; he then fainted, so that he had to be carried on a stretcher the remainder of the distance, about 400 yards, to the hospital. On admission, he was found to be labouring under the following injuries:—1st, A gun-shot wound of the left shoulder:—the ball entering anteriorly about three inches below the joint, passed backwards and upwards, and made its exit posteriorly and nearly opposite the joint, causing in its passage a comminuted fracture of the humerus extending into the joint; there was also extensive laceration of the soft parts. 2nd, A gun-shot wound of the right nates:—the ball, entering posteriorly about an inch and a half from the mesian line, and opposite the fourth sacral vertebra, fractured a portion of the sacrum; it then passed forwards with a little inclination inwards and downwards, came out anteriorly between the right thigh and the scrotum, then entered the latter, and finally made its exit anteriorly, passing between the testes, slightly injuring the posterior portion of the right testicle in its passage. 3rd, A transverse

wound of the forehead, evidently caused by a blow from a stone, which he must have received sideways, as I could distinctly trace with my finger a furrow of about an inch and a half long in the outer table of the cranium. No fracture could, however, be detected.

The state of the arm being such that it was impossible to save any portion of it, amputation at the shoulder-joint was decided upon; but I had to wait until sufficient re-action had set in, administering during the interval the usual stimulants, such as brandy and water, ammonia, &c. The ordinary flap operation, making the deltoid muscle the external flap, was that which I adopted; the patient did not lose three ounces of blood, probably in consequence of the hemorrhage which had taken place before he reached the hospital. The main artery alone had to be tied; the flaps were kept in position by means of two points of suture and two strips of adhesive plaster. One strip of plaster sufficed for the forehead; he was then put to bed, and the wounds of the nates, thigh, and scrotum dressed with simple ointment. No cold application was applied to the stump, as I feared, from his weak condition, it might retard the necessary degree of inflammatory re-action requisite for union by the first intention. At night he took a draught containing twenty-five minims of tincture of opium, in an ounce and a half of camphor mixture.

January 14th. This morning he appeared in excellent spirits, having had a very refreshing sleep during the night; the shoulder still feels cool, and he does not complain of much pain in it; but he complains of great inconvenience, in consequence of being obliged to lie constantly on the right side, from the position of his wounds. Milk diet.

15th. About noon the stump felt a little hot, when a cold lotion was applied to the part.

16th. He felt very comfortable this morning; scarcely any pain in the stump, from which, however, there was a slight discharge, apparently from the suppuration of the cartilage of the glenoid cavity; there was also a slight discharge from the nates. I took away the two points of suture, but did not disturb the adhesive plaster.

17th. The discharge was slightly increased both from the stump and nates; there was also a slight discharge this morning from the scrotum; the bowels being confined, he was ordered an oil draught.

18th. The discharge from the before-mentioned parts was still increasing; the wound in the forehead had commenced to suppurate: cold lotion discontinued at the shoulder, and the stump dressed with simple ointment.

22nd. The discharge from the several parts was still increasing: he had as yet experienced no uneasiness from any of the wounds, and was in the enjoyment of excellent health. Milk diet, with four ounces of meat.

23rd. No change. Allowance of food increased to half diet.

26th. The discharge from the nates had increased considerably, and I could feel with the probe a large piece of the sacrum denuded of its periosteum; but it did not appear to be perfectly detached

from its bony connexions; the wounds of the scrotum and thigh were doing well; the discharge from the stump was getting thinner and less abundant; there was still a slight discharge from the forehead. Half diet, with two ounces of rice.

30th. Continued much in the same state; getting stronger; bowels confined. Ordered house medicine.

February 10th. Improving rapidly. Cicatrization had commenced at several points at the edges of the flap, though there was still a slight discharge; the ligature had not as yet come away; the granulations appeared to have united around it. A small piece of bone came away in the dressing from the nates, from which the discharge was still abundant; the wounds of the scrotum were healing fast; the discharge from the forehead still continuing. Half diet, rice omitted.

18th. In the same state as on last report. Two more pieces of bone came away from the nates.

27th. The appearance of the wound in the forehead indicated the exfoliation of a portion of the outer table of the cranium. The scrotum quite well. The greater part of the flap of the shoulder had united; I cut the ligature off short, as there appeared to be no likelihood of its coming away; several more pieces of bone had come away from the nates.

March 2nd. Beginning to walk about a little; not having been able to lean the weight of the body on the right leg until now: the pieces of bone have ceased to come away from the nates.

10th. A piece of bone from the anterior table of the cranium, about one inch long by one-third of an inch broad, was extracted from the wound in the forehead: in other respects he continued the same as at last report.

21st. The shoulder quite healed, as also the wound in the forehead. The discharge from the nates still continued, and appeared to be chiefly composed of disintegrated bone; the dead portion of bone not as yet detached from the sacrum.

23rd. A hard and painful swelling had, within the last two days, formed between the tuber ischii and the anus. A poultice was applied to it.

27th. I could detect matter in the swelling, but very deeply seated; it caused great pain. I therefore cut down upon it, and gave exit to about an ounce of matter, which gave immediate relief. Through this opening I could detect with the probe the dead portion of the sacrum. Poultice directed to be still continued. From this time until he was discharged, which was in the beginning of July, he continued to gain strength in the right leg, though a slight discharge from the nates still continued.

I have considered the above case worthy of being placed on record, in consequence of its possessing some points both of interest and of rarity. The chief of these are:—its occurrence in a coloured man; the remarkable absence of a single bad symptom during the progress of the cure; and the only medical interference

required consisting in an opiate draught and two doses of opening medicine, notwithstanding the number and severity of the injuries he received, and also on account of the season of the year—it being midsummer, the thermometer often ranging 94° in the shade at Fort Beaufort, which is situated in a valley about seventy miles from the sea, being consequently deprived even of the refreshing midday sea breezes. I may, in fine, remark, that as the results of an experience acquired during the last six years in both the former and the latter Kaffir war, I have found coloured men bear the severest injuries better, and recover from them more quickly than those of European descent.

On the Internal Administration of Chloroform in Delirium Tremens. By Richard G. H. Butcher, F. R. C. S. I., Examiner on Anatomy and Physiology in the Royal College of Surgeons in Ireland, Surgeon to Mercer's Hospital, &c. &c.

The following very aggravated case of delirium tremens was admitted into Mercer's Hospital under my care, and treated most successfully by the *internal* administration of large doses of chloroform. I am not aware of the remedy having been used in England, and I thought I was the first who had tried it in this country; but from a recent conversation with Dr. Neligan, it appears he employed it some months ago in a case of delirium tremens to subdue extreme maniacal excitement which was present, and with success also. The practice, however, comes recommended from America[a]; and so far as a single case can speak, mine confirms the favourable report given of it, and still further goes widely to proclaim the powerful agency of the medicine over this inexplicable derangement of the nervous system.

William Magrath, aged 26 years, a powerful young man, by trade a wine-porter, was admitted into Mercer's Hospital June 25th, 1852; during the last four years has been in the habit of consuming large quantities of spirits of various kinds—wine, whisky, porter, &c. &c.—seldom to that extent as to produce stupid intoxication, but constantly keeping up for days together a state of the greatest nervous excitement; a day scarcely ever elapsed without a large quantity of stimulus being taken. About a fortnight before his admission he commenced to drink more freely than before, and, as I am informed by his wife, came home drunk every night for a week. He then, through the interposition of friends, was arrested in this career, and promised to abstain from ardent spirits altogether. His stomach became very irritable, nothing in the way of food would rest upon it; he was depressed and melancholy, and sunk into a state of temporary inaction or collapse: this condition did not last for more than forty-eight hours, when violent re-action of the nervous and vascular systems was fully established. Medical assistance was sought, but no medicine would stay upon the stomach, so that the symptoms gained

[a] American Journal of the Medical Sciences, January, 1852.

ground; when such was found to be the case, the patient's friends had him removed to hospital. When admitted, four days had elapsed from the time of his giving up the stimulus, and he had no sleep during that period; a train of symptoms consonant with the highest nervous irritation was present; his countenance was particularly anxious, with a wild expression; the pathognomonic symptom—tremor of the hands and tongue—fully established; his speech was hurried and uneven; he was quite irrational and wild, constantly looking around, apprehensive of some imaginary danger. Pulse, 120; surface of the body hot and burning, while his face was covered with perspiration and his hair drenched in sweat. He was put into bed, but would not remain quiet, getting up and constantly walking up and down the ward and corridor. He was ordered two grains of calomel and a grain of opium in pill, to be taken every third hour; he had taken three, but each was vomited almost immediately after being swallowed. I then tried morphia in combination with creasote and camphor—a grain of morphia, two drops of creasote, and an ounce of camphor mixture—given every third hour; the draughts were likewise rejected: if the patient only took a sup of cold water to moisten his parched mouth and lips, it was instantly vomited. On the following morning, the 26th, his condition was a great deal worse. He never stopped quiet for a moment from the time of his admission, at 10 o'clock on the previous morning, up to this time, a period of twenty-four hours; he was walking about all night with a keeper in attendance; his countenance was more wild, the eyes starting from his head; he was more delirious and more haunted with illusory apprehensions of a frightful nature; the tremor of the tongue was greatly increased, and now the lower extremities participated in it more violently than the upper. The case at this period was a very serious one. From the irritability of the stomach, opium in any form could not be got to rest upon it; as for the idea of administering repeated small opiate enemata in this powerful, restless, and uncontrollable young man, the practicability of it could not be entertained for a moment, though sanctioned by the high authority of Dupuytren: it may be said the object could be attained by force, but that I would not sanction. All through, it may be observed, I permitted the man to walk about the ward at will, closely watched. I have the greatest possible aversion to restraint in this disease. (I separate altogether from the present question the treatment of traumatic delirium after fractures.) I have seen a man in delirium tremens, from being held down and overpowered, thrown into epileptic convulsions; and I have seen a man die exhausted from ineffectual efforts to shake off and liberate himself from the strait waistcoat. From the satisfactory issue of the two cases reported in the periodical already referred to, I determined on a like practice—the internal administration of chloroform.

At 10 o'clock this morning, the 26th, I administered one drachm of pure chloroform in two ounces and a half of water. In an hour after swallowing it the patient became comparatively tranquil, and

could be persuaded to lie in bed. When in the recumbent posture, the spastic, short contractions of the muscles of the lower extremities, as well as those of the upper, were as marked as in the erect position.

11 o'clock.—He began to get drowsy, and slept for periods of ten and twelve minutes at a time. At a quarter before one o'clock he became fully affected by the medicine, and fell into a quiet, steady sleep; and on visiting him at 2 and 4 P. M., he was still in profound sleep, and continued so until 7 in the evening: during this long sleep of six hours he was calm and quiet; his pulse fell from 120, which it was in the morning, to 96, at which it remained; his respirations were between sixteen and twenty in the minute, and not louder than natural; the temperature of the body was exalted. All along heat was maintained to the feet, and a pure current of air kept circulating around him, the windows being open. On his awaking he was nearly quite sensible, and I took advantage of this pause to administer a full stimulant cathartic, consisting of six grains of calomel and ten of camphor, not only with the intention of freeing the bowels from accumulated matter, but likewise to guard against congestion of the brain. Orders were left, in case he should not sleep before ten, to administer half-a-drachm of chloroform in two ounces of camphor mixture.

27th, 10 A. M.—The patient went to sleep almost immediately after swallowing the bolus on last evening, so he did not require the chloroform draught; his bowels were opened three times very freely during the night, and his condition is in every way greatly improved. He is quite rational, and answers every question sensibly; his pulse 96, considerable volume; skin cool. After being interrogated, he quietly turned on his side and went to sleep.

3 P.M.—His bowels have been several times opened since morning, yet his pulse has risen to 110; the temperature of the body is also increased; he is hot and burning; altogether he is excited, and the fear of horrible objects around him has returned. On the presence of these symptoms, I at once repeated the full chloroform draught. Shortly after, he took a large drink of tea which was inadvertently left beside his bed, which produced vomiting immediately: however, I was satisfied to let him remain without any further medicine until evening.

9 P. M.—Since my last visit, the patient has slept, at short intervals, for one and two hours at a time. Pulse still up to 110. Ordered the chloroform draught, one drachm to two ounces and a half of camphor mixture, to be repeated.

28th.—After the patient had taken the draught last night, he fell into a quiet sleep, which continued uninterrupted until eight o'clock this morning. He awoke quite collected and rational; his pulse 80; skin cool; his tongue and extremities quite free from tremor; and he feels in every respect well; his appetite has returned, and all food is retained on the stomach. Ordered a grain of morphia in an ounce of camphor mixture, to be given at night.

29th.—This morning the patient is quite restored; he is sitting up eating his breakfast heartily in bed. In short, he is quite convalescent, and only requires a little nourishment to remove the debility consequent upon so severe a struggle.

In reference to the administration of the chloroform in the foregoing case, there is one point which solicits our closest attention—namely, the remarkable lowering of the pulse when the perfect effect of the medicine was produced. The pulse, in fact, might form the index to direct the practitioner as to the propriety of a repetition of the dose. Again, as a precautionary measure, I consider it desirable to keep heat to the feet, and a current of pure air circulating around the bed and through the apartment in which the patient lies.

Case of Rupture of the Heart from External Violence. By Arthur Leared, M. B. T. C. D., M. R. I. A., &c.

The following case of a somewhat rare occurrence, transcribed from my note-book, may prove of interest. On the 23rd of October, 1850, I was directed, by order of the coroner of my district, to make a *post-mortem* examination of the body of a man of about sixty years of age. He had been engaged alone in throwing down the house of a peasant, the thick mud wall of which having been undermined fell upon him, and under which his body was found long after life had been extinct. It was much disfigured, more than one fracture of the extremities having occurred, and there was great contusion about the face; the left clavicle was fractured, and there was copious effusion of blood in the adjoining tissues: the second, third, fourth, and fifth ribs of the left side were fractured about their centres, the broken extremities having been driven in upon the lung, which accounted for the emphysematous condition of this part of the thorax. On laying open the pericardium, which was itself entire, it was found to be full of coagulated blood: further examination showed this to have proceeded from a rupture of the heart occurring in its right auricle. The rent extended in a parallel direction from the root of the aorta, immediately beneath the auricular appendix, and was about two inches in extent; the auricle was dilated, its walls being thinned, circumstances that contributed to the occurrence of rupture. A general hypertrophy of the heart existed; it weighed, when empty, $11\frac{3}{4}$ ounces. The aorta was dilated, and about its orifice inferior to the valve there were some atheromatous deposits. The rupture in this instance was situated in the cavity in which it has been most usually observed when induced by external violence.

Cases of Disease of the Cerebro-spinal System. By T. Purefoy, M.D., Medical Attendant of the Cloughjordan Dispensary.

Case I.—*Hemiplegia, with Anomalous Symptoms.*—M. D., aged 40, a stoutly-made labourer, of middle stature, regular and laborious

habits, suffered from an attack of fever about ten years since, after which his convalescence was slow and incomplete. He had been afterwards habitually liable to headach and attacks of giddiness, accompanied by a sense of weakness. He resumed his usual habits, however, but, to use his own expressive language, "he was never the same man" as before his illness. Two years after the fever, he was attacked with hemiplegia of the right side, which was treated by active purgatives and blistering, as the patient would not submit to bleeding. Recovery from this attack was slow, but so perfect that he was again able to follow the plough. In November, 1851, just eight years from the first attack of hemiplegia, there was a recurrence of the disease. His symptoms now were, slight numbness, and loss of sensibility of the right arm, whilst the motive powers were not much impaired, and partial loss of sensibility and motion of the right leg and thigh, so that he staggers as he walks, dragging the leg after him, which is so feeble as to be unequal, alone, to bear the weight of the body. Pulse 84, soft, moderately full; no pain or other decided symptom of local organic disease of the brain or spinal marrow; the animal functions are performed normally, yet the expression of countenance is peculiar, being dull, inanimate, and vacant.

He was now bled from the arm, and upon losing twelve ounces of blood became faint, and was slow in recovering. An efficient course of purgative medicine was subsequently employed, by which plan of treatment the patient's health was so far restored that he again resumed his ordinary avocations. In the middle of March, 1852, whilst engaged in ploughing, symptoms of paraplegia came on, and rapidly increased, so that in a few days he became wholly unable to walk unsupported. The sensibility and mobility of the lower limbs were much impaired; pulse 84, soft and compressible; no local pain; bowels constipated; expulsive powers of the bladder weakened, so that it required a prolonged effort to expel the urine. A few ounces of blood being now taken from the loins by cupping, faintness ensued, when the operation was discontinued; blisters to the loins and purgatives were subsequently employed, and followed by the use of bitters and mild tonics when the regular action of the bowels had been restored.

Under this treatment his health improved considerably, but, unfortunately, having become negligent in the observance of the rules of treatment prescribed, he again suffered from an attack of unusual severity in the month of April, which came on suddenly whilst out of doors, so that he fell to the ground in a state of total insensibility, and was carried home. It was after the recurrence of this fourth attack that the *anomalous* symptoms manifested themselves. His appearance became idiotic; he lost nearly all voluntary control over both the lower and upper extremities; articulation was so imperfect that he was scarcely intelligible; whilst deglutition was performed by irregular and painful efforts. The bladder was not paralyzed, but the bowels were obstinately constipated; the condition of the tongue and pulse were the same as in his former attacks. The

anomalous symptoms alluded to above consisted in the rather unusual manifestation of some form of mental malady. The patient became desponding, gloomy, and averse to all society; was drowsy, and much disposed to sleep, but when aroused by the approach of any friend, he suddenly burst forth into an apparently involuntary outbreak of loud and piteous wailing, deeply deploring the weakened and helpless state to which he had been reduced. His voice was quite altered: it was now high-toned, shrill, and feminine; and so irresistible was the tendency to weep, that every word uttered was accompanied by a flood of tears, yet his answers were perfectly rational, nor did there appear to be any impairment of intellect. The vague expression of countenance seemed to result chiefly from the irregular action of the muscles of the eyeball, giving rise to strabismus, whilst the pupils were dilated and sluggish, at the same time that a large quantity of clear, salivary fluid flowed uninterruptedly from his open mouth.

The treatment now consisted in the diligent use of active purgative medicine, together with a regular succession of blisters to the nape of the neck. This treatment was persisted in for three weeks, with very decided improvement in the general symptoms. At the end of this time the blisters were applied less frequently; chalybeates and other tonics cautiously used, purgatives being occasionally exhibited, whilst the patient was kept in the open dry air as much as possible. Improvement has been progressive under a tonic plan of treatment; and this poor man is now able to walk some miles, articulates his words perfectly, and has quite overcome his crying propensity.

Case II.—*Cerebro-spinal Irritation.*—G. G., son of a farmer, aged 12, of spare and delicate form, but healthy, active in his habits, and intelligent, in the month of February, 1851, whilst working in a field and perspiring freely, was suddenly attacked by acute and overcoming pain in the head, which obliged him to stretch himself upon the ground in agony, whilst he begged that he might be assisted to his home. He was immediately removed to his father's house, which was near, when he became sick, threw up the food eaten at breakfast, and subsequently some clotted grumous blood. He was put to bed, suffering under most severe pain of the head, complete prostration of strength, and distressing sense of coldness generally; whilst the face, hands, and feet were much below the natural temperature. When first visited upon the 21st February, he had been three days ill; he now lay upon his side, the head thrown backwards, and the spinal column slightly flexed in the same direction; the extremities were cold and drawn upwards; whilst he studiously avoided every movement of his body from the position in which he lay. His face was pale, and expressive of deep suffering; appearance of the eye nearly natural, pupils dilated and sluggish; no fever; tongue moist and clean; pulse 48, full, and moderately firm; skin dry; secretion of urine scanty. He complains incessantly of the bursting pain in his head; objects strongly to light or noise;

has had no return of vomiting after the first attack, but the bowels have been confined for the last two days.

Treatment.—He was now bled to five ounces from the arm: faintness and failing of the pulse were thus induced, yet the pain continued unrelieved. Warm flannels were now rolled around the extremities, and the forehead lightly covered with cloths wrung out of tepid water, and frequently renewed. The nape of the neck had been blistered before the visit, but without any benefit. Calomel and jalap were given as a purgative every two hours until the bowels were relieved, and a diaphoretic mixture, containing nitre, solution of tartar emetic, and spirit of nitrous ether, was administered in small quantity between each dose of the purgative medicine.

22nd. Headach unabated; bowels confined. Three leeches were applied behind the ears, and the medicines continued.

23rd. Pain extending from the nape of the neck; mercurial diarrhœa; head scarcely relieved; intellect sound, neither delirium nor convulsions; pulse 52; slight febrile re-action. Three leeches were ordered to the nape of the neck subsequently; and a blister over the spine at its upper portion. Small doses of hydrargyum cum creta were taken every third hour, and the diaphoretic mixture continued.

25th. Decidedly better; pain of head much less severe; and the pain of back most felt at the sacrum.

26th. Progressively improving; pulse 84; all headach gone. The child is now cheerful, expresses his gratitude at being relieved from pain; is willing to take food moderately; has lost all sense of coldness, and feels himself to be comfortably warm, whilst the skin is soft, and of a natural heat. From this date no special medical treatment was required; the debility consequent upon the illness was quickly overcome, and the little patient was soon re-established in his usual good health and spirits.

In the first of these cases we have an interesting illustration afforded of the influence which an attack of the ordinary epidemic fever of this country has in predisposing the subject of it to the occurrence of, perhaps, some more formidable disease at a subsequent period. Hence arises an urgent necessity for the observance of great care and watchfulness during the period of convalescence; and also, for very mature deliberation before pronouncing a fever patient to be "wholly out of danger." Convalescence here was never complete; and although no symptom of cerebro-spinal disease occurred for so long a period after recovery from fever, yet I think we may justly infer that the attack of fever was, in the present instance, the predisposing cause of the subsequent paralytic seizure.

It is probable, from the occupation of this man, that exposure to the influence of cold and rain, whilst heated by labour, may have been the exciting cause of the attack in the first instance. The success which attended the employment of local bleeding and blistering, with the aid of active purgatives, would lead to the conclusion that the paralytic symptoms were produced rather by a congestive, than inflammatory affection of the brain or spinal marrow.

The propriety and advantage of modifying the plan of general treatment according to the several peculiar symptoms, and the duration and progress of the case, were also satisfactorily shown,—for, whilst a strictly antiphlogistic treatment was found successful in the early period of the disease, a directly opposite one was of marked benefit in its more advanced stage, when the predominant symptoms indicated, not merely a deficiency of nervous and muscular energy, but also a remarkable prostration of the mental powers. Twice only have I met with paralysis, accompanied by this *irresistible desire to weep.* In the first case, it occurred in a patient who was hemiplegic, bedridden, and suffering under valvular disease of the heart, and who shortly afterwards died apoplectic. In the present case, I was agreeably disappointed by the unexpected improvement in my patient, having looked upon it as one that would terminate fatally. Although this man is now able to take long walks and work moderately, of course, his life must be considered as of unusual uncertainty, and probably he will ultimately be the victim of an apoplectic seizure. In the absence of a better name, I would propose to call the peculiar form of paralysis here described, *the weeping palsy.*

The history, symptoms, and progress of the second case, would at once lead to the conclusion, that sudden suppression of perspiration by a harsh cold wind, whilst the body was overheated, favoured the occurrence of acute congestion of the brain and spinal marrow. The early symptoms were, sudden acute headach, prostration of the vital powers, coldness of the surface of the body generally, vomiting, and remarkable slowness and sluggishness of the pulse. Subsequently, the pain extended to the spine, the head was thrown backwards, and the spinal column flexed, whilst the movements of the body aggravated the pain. The usual symptoms of an inflammatory affection of the brain, such as fever, delirium, suffusion of the eyes, intolerance of noise and light, frequency of pulse, &c., were absent altogether, whilst the blood taken from the arm was neither buffed nor cupped. Although the dry skin, low temperature, and sense of coldness, together with the absence of those symptoms which usually indicate re-action in the system seemed to contra-indicate bleeding, yet the progress and favourable termination of this case sufficiently justify the propriety of employing this remedy, whilst its good effect in relieving congestion of the internal organs was further promoted by antimonial diaphoretics, and calomel and jalap as a purgative. The disease was remarkable as to the suddenness of its invasion, the shortness of its course, and the complete relief of the urgent symptoms which followed upon the treatment pursued.

The total absence of fever, depression of the vital powers to a certain extent, and obstinate constipation of the bowels, taken in connexion with the character of the pain present, would incline me to describe this case as one of acute congestion, rather than inflammation of the brain and spinal marrow. Hematemesis was probably the result of a congested state of the gastric mucous membrane, which must have been in a high state of functional activity

when the supposed exciting cause, cold, came into operation, as the boy had only just breakfasted when he went to work. In the third Number of the new series of this Journal, there is published a very interesting paper by Dr. Mayne, upon a form of disease, named cerebro-spinal arachnitis, of modern date, and but lately observed upon the Continent and in Great Britain. The preceding case is, in its leading symptoms, so similar with the disease described by Dr. Mayne, that their identity would seem to be established, although I have preferred employing a name more strictly in accordance with the symptoms which occurred during the progress of the disease.

Case of Femoro-Popliteal Aneurism, cured by Compression. By Robert R. Gelston, M. D., Surgeon to Barrington's Hospital and the Limerick County Infirmary; Physician to the Gaol and St. John's Fever Hospital, and Consulting Physician to the Lying-in Hospital.

The patient in this instance was a man of highly nervous temperament, of a wretched, squalid appearance, and in a perfectly anemic state; on the whole, totally unfitted for any operative proceeding.

The following is a brief history of the case:—

Thomas O'Neil, a tailor, aged 37, was admitted into Barrington's Hospital on the 19th of February, 1852, with a pulsating tumour of an ovoid or fusiform shape, about the size of a goose-egg, situated immediately below the opening in the tendon of the triceps muscle of the right thigh, extending from thence downwards towards the popliteal space, constituting a femoro-popliteal aneurism.

Though a man of intemperate habits, he had nevertheless enjoyed very good health up to about four months prior to his admission, when he received a severe injury to the right knee by falling against the edge of a counter. The pain on this occasion was very severe, but, as it subsided in a few days, he took no further notice of the occurrence, and worked on as usual at his trade until about five weeks ago, when his attention was attracted by a small pulsating tumour, situated above and to the inner side of the right knee-joint, or the lower portion of the thigh, which, gradually increasing, induced him to seek for advice.

Feb. 19th. On presenting himself at the hospital, the following was the condition of the parts:—The affected leg and foot were numb, and he complained of much pain in and about the knee, which he said was particularly severe at night. The temperature of the foot was much lower than that of the opposite one, and neither the anterior nor posterior tibial arteries could be felt in their usual situations.

On examining the popliteal space, a resisting tumour was apparent, of a fusiform or ovoid shape, about three inches in length by two and a quarter in width, pulsating strongly; marked by a *bruit de soufflet* synchronous with the influx of blood into the aneurismal sac, and accompanied also by the addition of a continued venous

murmur, quite characteristic of his anemic state. Pressure on the femoral artery completely arrested the flow of blood into the sac, and when kept up for a very short time the latter became perfectly empty, and its walls collapsed. The pulse at the wrist was 103; the patient appeared highly hysterical and nervous to a degree. The glands in the groin were much enlarged and tender, but the thigh was free from any cicatrix of bubo. The heart's action was feeble, with an anemic *soufflet* accompanying the first sound; its valvular structure was apparently unimpaired. He was ordered a generous but non-stimulant diet; the limb to be wrapped in flannel, and to be placed in a semiflexed and somewhat elevated position.

On the 26th, the part having been shaved on the previous day, prior to the adoption of regular compression a weight of 7 lbs. was placed upon the artery in the groin as preliminary treatment. This diminished, but did not entirely control, the circulation through the sac: he was directed to take an anodyne, consisting of thirty drops of the solution of the acetate of morphia in camphor mixture at night.

On the 1st of March, in addition to the above treatment, he was ordered five grains of the saccharated carbonate of iron thrice a day; pressure by the weight to be continued, but only at intervals.

On the 6th, the patient's general health being somewhat improved, compression was regularly commenced; and at twelve o'clock at noon Carte's *elastic* compressor was placed on the artery in the groin, and worn until four o'clock, P.M., when, from causing pain in consequence of the very tender state of the inguinal glands, it was relaxed, and the circular or thigh compressor of the same surgeon placed on the limb where the artery winds round the femur in its lower third. This was worn until the visiting hour on the following morning, when the hip apparatus was resumed; but still, owing to the very tender state of the inguinal glands, it was removed, and the circular compressor was had recourse to, and worn, *at intervals*, during that day and the succeeding night.

On the 9th, the pulsation in the sac had somewhat diminished, whilst the temperature of the limb had sensibly increased.

From last report, up to the 2d of April, pressure was continued at intervals, but never so as to fatigue the patient or cut off completely the flow of blood into the sac; and the same constitutional treatment was steadily followed up, but without any very marked improvement being perceptible either in the size or consistence of the tumour, or the force of the circulation through it, as, from the tender state of the glands in the groin, and the extreme irritability and nervous temperament of the patient, even a moderate amount of pressure could not be maintained for two hours at a time. Still the case was progressing, for on examining the aneurism this morning it was quite evident that a deposit of fibrine had taken place, as the sac felt much firmer, and the pulsation was now decidedly diminished; in addition to which, the other indications of the curative process were manifested, such as the patient's complaining of sharp, shooting pains coursing down the leg and foot, and the latter feeling

sensibly warmer; the internal articular arteries being now perceptible over the inner condyle:—all illustrative of the increased collateral circulation being established, and to which practical points particular attention has been directed by Dr. Bellingham and Mr. Tufnell in their writings on this subject.

On the 6th of April the tumour had sensibly diminished, and was considerably firmer, though the pressure had been continued only at intervals as before. To-day, as he complained of pains in the loins from weakness and lying in bed, he was directed to lie on a sofa during the day, and to wear the compressor outside his trousers; and not having slept well, and being still very nervous, he was ordered to take a mixture consisting of sulphate of quina and tincture of valerian in camphor mixture thrice a day.

On the 10th of April he had considerably improved since he commenced the tonic mixture. He slept soundly at night, was much less irritable, and could now, for the first time since the commencement of the treatment, bear continuous pressure for some hours consecutively, which is to be entirely attributed to the improved state of his general health, causing the glands in the groin to be absorbed. The *bruit* in the sac had disappeared, and the whole aspect of the case had within the last three days so much improved as to give every confidence of success.

On the 16th the dimensions of the tumour had considerably decreased, and the collateral circulation was perfectly established through the articular vessels, which could now be traced pulsating plainly round the knee-joint; the temperature of the limb was the same as the sound one. Pressure continued.

On the 17th, at the morning visit, pulsation could be felt in the sac; but at one o'clock in the afternoon it finally ceased.

When the extreme debility and highly nervous condition of this patient are taken into consideration, I am quite satisfied it will be admitted that one more unsuited for the ligature could not well be imagined; and had I followed the old routine practice of securing the vessel at any period from his admission into hospital on the 19th of February up to between the 6th and 10th of April following, I have little hesitation in saying, that in all probability gangrene would have been the result. This being my decided opinion, I determined to follow that line of treatment which, though individually not thoroughly versed in, still, as it seemed to afford my patient the most reasonable chance of cure, and emanating, as it does, from a body of sound practical men such as the Dublin surgeons are known to be, I hesitated not to adopt it, with every confidence in its ultimate success, especially bearing in mind the anemic state of my patient,—that should the vitality of the limb decrease, from the want of its due supply of blood, the patient had it in his power to restore it at once, by relaxing the compressor; and in this one respect, if not in some others, this mode of treatment seems to me to have a decided superiority over the ligature,—for where the latter is placed around the vessel, the main supply of

blood is cut off at once, and cannot, as with the former, be again restored at the will of the surgeon. I consider this a most important practical point, and one that should never be underrated; and likewise, that during the use of compression, the collateral circulation was being established, thus giving the patient a far better chance of perfect recovery, even supposing that it became necessary to have recourse to deligation as a *dernier ressort.* But certain am I that this will seldom be the case, if the surgeon frees his mind from prejudice, and steadily and fairly pursues compression; but to do this, with the expectation of a beneficial result, he must necessarily have confidence in it.

It may be contended that this patient was a very long time under cure. So at first sight it would appear, and I make no doubt the case might have proved a failure in the hands of some surgeons, as its progress during the first four or five weeks was such as to be anything but encouraging, day after day apparently passing without any very marked change in the local disease, so much so, indeed, that some of those surgeons who witnessed it expressed their doubts of its ever succeeding. But let us analyze the time, and see the result. Pressure was commenced seven days after his admission into hospital, that is, on the 26th of February, and the cure was completed on the 17th of April, fifty days, or seven weeks and one day.

But, as before stated, I am confident that no surgeon, even of moderate experience, would have attempted to tie the femoral artery in this case until between the 6th and 10th of April, when the patient began to recover from his wretched and anemic condition; and indeed compression seemed to have very little effect upon the tumour until about this time, but, nevertheless, benefit was resulting, for during the interval the collateral vessels were dilating, which was plainly manifested not only by the elevation of temperarature of the leg and foot, but by their being felt pulsating around the knee-joint.

Let us then take from the 6th to the 10th of April as the period at which he was in a fit state for the operation, and suppose that the vessel was accordingly secured in the usual way. Under such circumstances, may I ask, where is the surgeon who would expect to see his patient out of bed in the course of the following week or ten days,—the aneurismal tumour not only perfectly solid, but commencing rapidly to be absorbed, the collateral circulation perfectly established, and in fact, the man, instead of lying in bed, with an open wound, and a ligature still to come away, dressed and reclining on a sofa, free from suffering, and all his nervous irritability removed?

As this was the first case of aneurism in which compression had been employed in the south of Ireland, it was consequently visited by many medical men, who were anxious to see this mode of treatment carried out.

The instruments used in the foregoing case were those which

experience has found to be best adapted, namely, "Carte's Elastic Compressors." They answered admirably the intentions of the surgeon, and when the patient became acquainted with their use (which was in a very short time) were in a great measure left to his own management, of relaxing or making pressure, according to his feelings, thus preventing any chance of producing gangrene, which, in such an anemic patient, was to be apprehended.

He left the hospital on the 8th of May, and returned to his trade. He called on me on May 11th, walking very well, with the assistance of only a single slight stick. I found the tumour considerably diminished, the hamstring tendons easily felt, his general health greatly improved, indeed so much so, that no one who saw him on the day of admission could now believe him to be the same person,—his nervousness gone, freshness and colour restored to his cheeks, and from miserable leanness, become fat and muscular.

I met my patient casually in the beginning of this month (July) walking to his employer's, and examined the limb very carefully, and also the popliteal space. All evidence of the tumour was now removed, the thigh and leg restored to their former plumpness, and the articular arteries very large.

On a Form of Partial Atrophy (Trophonéurose *of* Romberg). By Dr. Ch. Lasègue.

Carswell was certainly the first to systematize the study of atrophies. The observations which had been made on this subject, although numerous, were, previously to the appearance of his work, unconnected; and this variety of pathological change had been generally mentioned as only an incidental symptom in more serious affections. Carswell[a] arranged scientifically the various lesions which might arise in consequence of a retardation, an arrest, or even an inverted action in nutrition; and, at the same time that he assigned their causes, pointed out the several forms of atrophies, the parts most likely to be affected by them, as well as the modifications they produce in the texture of organs. Since that time, the divisions he adopted have been almost always adhered to.

Among the most characteristic varieties must be enumerated the atrophies which arise from some derangement of the nervous influence. Every author who has treated of paralytic affections has directed attention to the tardiness of the development of paralyzed limbs, and to the emaciation of muscles condemned to a state of inaction. The gradual atrophies which successively invade the entire body, giving to some old people a repulsive, skeleton-like appearance, had also been described under different denominations. Of this variety, Lobstein had brought forward an example, remarkable

[a] Illustrations of the Elementary Forms of Disease, 1836.

both for the extent of the affection and for the early age at which it occurred. It was natural to suppose that in cases of this kind a lesion of the nervous system, different from that which produces paralysis, but still not clearly understood, was the cause of the emaciation. The very original essay of Dr. Aran has thrown a new light upon the question, and furnishes valuable materials for the history of classified atrophies[a].

However, if it were possible to venture an explanation, and to refer the arrests of local nutrition to the want of exercise of the particular organs, and general atrophies to an ill-defined feebleness of innervation, facts of another kind would escape from our hypotheses, and would outstrip our physiological previsions. Dr. Rey, of Bourdeaux, made, in 1834, a curious though very incomplete observation[b] in reference to the case of a man aged 35, who was admitted into the Hôpital St. André, to be treated for chronic ophthalmia of the right eye. This man, low in stature, presenting no trace of scrofulous affection, was nearly idiotic. All the parts of the body right of the median line were atrophied: the skull on that side appeared to be depressed; the eye-brow and the zygomatic arch were less prominent; the limbs were shorter and smaller than on the left; the hair was scanty, and fine in texture; the senses were blunted. On the left side, on the contrary, all the nervous functions were regularly performed. The patient could give no history of himself. The author considered what part the nervous system might perform in producing so singular a duplication of nutrition, and how far it might be reasonable to suppose the existence of a *nutritive* paralysis coincident with the abolition of motion and of sensation. This observation, we must allow, brought only a very questionable element to the solution of the problem. Parry gives much more valuable information, in quoting the case of a woman in whom he had observed an atrophy, likewise unilateral, without concomitant paralysis, and without lesion of any kind which could afford an explanation of the affection[c].

These first data had continued disregarded, when an eminent professor at Berlin, to whom we are indebted for his important labours on the subject of nervous diseases,—Dr. Romberg, directed attention to this form of atrophy, hitherto misunderstood, if not unknown. Several of his pupils, and some physicians of the north of Germany, added new facts to those of which Romberg had been a witness. It will not be uninteresting to recapitulate the little that is known about this singular affection.

It was in his Clinical Researches, published in 1846[d], that the author first mentioned the disease. under the designation of "a new form of atrophy of the face." His patient, who was twenty-eight years old when she entered the Polyclinical Institution of Berlin,

[a] Archives Générales de Médecine, September and October, 1850.
[b] Sur la Pathogénie de quelques Affections de l'Axe Cérébro-Spinal.
[c] Collections from unpublished Writings. London: 1825.
[d] Klinische Ergebnisse gesammelt, von Dr. Henoch.

had at the age of thirteen an attack of tertiary fever, from which she soon recovered; at fifteen she had probably laboured under scarlatina, which was followed by the serious sequelæ for which she now sought advice. Her general health had been in other respects quite re-established, but her state at the time of her admission into hospital was reported as follows:—The right side of the face is plump and ruddy, while the left is wrinkled and dried up like that of an old woman; the median line forms an exact demarcation between the healthy and morbidly affected parts; the forehead is less convex on the left side, the hair is thin, the eyebrow is more salient, but nearly deprived of hairs, the eyelashes are gone, the eyelid is emaciated, the caruncula is pale and contracted; the left side of the nose, mouth, and chin, presents a remarkable atrophy; the proportion of the two sides of the face, when compared, is as 4 to 10; the atrophy extends to the left tonsil and the left half of the uvula; there is no lesion of sensation or motion, nor is there any alteration in the secretion of the tears, of the saliva, or of the cutaneous perspiration. The patient was repeatedly examined down to the year 1851, but the disease had not diminished; on the contrary, the atrophy had manifestly progressed. Dr. Schott selected this case as the subject of his thesis, describing it with the most exact and interesting minuteness. He designated the affection *trophoneurosis*[a].

Attention having been once aroused, it was to be expected that new observers should witness analogous cases. Drs. Hueter and Axman accordingly collected some examples[b], and Romberg himself published two cases, which complete and confirm his first researches[c]. I will select the most characteristic from all these cases; the novelty of the subject will be my excuse for the length of the extract.

Case I.—"Otto Schwann, aged nine and a half years, admitted into hospital on the 24th June, 1847. When six years old he had measles without any complication; at seven his present affection commenced, and now exhibits the following characters:—

"The lesions are confined to the left side of the face and to the left submaxillary region, and consist in an alteration of form and in an anomalous deposit of pigment. In the inferior half of the face, where the modification is most perceptible, the sound is separated from the morbidly affected tissue by a well-defined line, which, commencing at the septum naris, descends along the upper and lower lips, and so reaches the larynx, where it gradually disappears; it is but slightly marked on the dorsum of the nose, becomes again distinct upon the forehead, and is lost among the hair.

"From this line the deposit of a yellowish-grey pigment commences, and extends inferiorly to the angle of the jaw, divided here and there by portions of healthy skin; the discoloration varies in intensity, and exists in irregular patches on the upper part of the

[a] Atrophia singularum partium corporis, quæ sine causa cognita apparet, trophoneurosis est. Marburgis, 1851.

[b] Singularis cujusdam atrophiæ casus nonnulli, 1848.

[c] Klinische Wahrnehmungen, 1851.

face. On the forehead a similar alteration is formed; the discoloured tissues are glossy and greasy-looking.

"Even at first sight the left side of the face is in every sense diminished. The atrophy ceases at the median line; on the forehead the convexity is less marked; the lower jaw is more flattened; the point of the chin is bevelled off towards the left; the bony mass of the inferior jaw is lessened; the upper jaw has preserved its normal dimensions.

"The atrophy of the soft parts is not less defined; it is particularly evident on the left portion of both lips. On examining the mucous membrane, a white line is observed, terminating abruptly at the edges, in the situation of the frænum of the upper lip; the thickness of the atrophied parts of the lips is about half that of the unaffected portions; the mouth cannot be completely closed at the left side, but leaves, when the attempt is made, an oval opening. The tissue of the lips is hard and coriaceous, without fold or depression. The mucous membrane is deeply corrugated, and has preserved its normal red colour. At the anterior part of the lower jaw, the soft parts are immoveable on the bones; their adhesion to the latter is more marked, as the deposit of pigment is more considerable. The left ala of the nose is attenuated and sharper; the nasal orifice is larger and rounder; the eyelids, and particularly the tarsal cartilage, are likewise attenuated. There is complete absence of the eyelashes, from the inner angle of the eye to the middle of the lids. The masseter muscle on the left side is but little developed; it is impossible to estimate the form of the other muscles of the face.

"The hair is very thin, in some parts altogether wanting; the left eyebrow is almost entirely devoid of it. The left half of the tongue is narrower than the right, and is more or less deeply furrowed. There is no change in the uvula; the palatine arch seems a little more concave; the teeth are irregular; the left carotid beats, perhaps, less strongly than the right; the pulsation of the temporal arteries are equal on both sides.

"As to the functions, the most careful examination fails to discover the slightest difference: the sight, hearing, smell, taste, sensibility, and motion, are equal on both sides; mastication is unimpaired; the articulation distinct; the secretions and temperature are unaffected.

"In the absence of more positive indications, treatment by electricity was adopted; but although carefully persevered in for three years, it failed to produce any beneficial result. Medicine and frictions appeared to be equally inefficacious."

Like the author, I abstain from all commentary on the foregoing. The following case, occurring in an adult who was able to give a more exact account of the development and antecedents of the disease, presents some curious particulars. The atrophy was preceded by an affection of the trigemini, by contractions of the muscles engaged in mastication, and by a hyperæsthesia of the cutaneous branches of the fifth pair.

Case II.—"A. N., thirty-two years of age. When seven years old, suffered from spasmodic contractions of the masticating muscles with chattering of the teeth, caused by repeated rigors, recurring in irregular paroxysms, and lasting for some minutes. From the age of fourteen, these convulsive movements diminished in frequency and intensity. The sensibility of the skin continues exaggerated, as it was at the first occurrence of the symptoms. The atrophy appears to have commenced at the same time, and now (1848) presents the following characters:—

"The left half of the face is flattened; the temporal depression is nearly twice as great as that on the right side; the patient of his own accord states, that the bone appears to him to be attenuated, and that it crepitates like a sheet of paper when pressure is made in this situation. The skin rests upon the muscles and bones, without the interposition of a layer of fat; the muscles, though diminished in bulk, have lost nothing of their contractility. There is no hair on the temporal region, no whisker on the left side; the beard is equally abundant on both sides of the chin. The left half of the inferior jaw is shorter and thinner than the right; the healthy side is separated from the other by a kind of furrow. The lips are thin; the fifth inferior molar tooth is wanting on the left side. The left portion of the tongue is but half as wide and thick as the right; the uvula is similarly circumstanced; there is no functional difference between the two sides; the left cheek, measured as accurately as possible, is exactly half as thick as the right."

If we add to these three cases those of a young girl in whom the atrophy was confined to the temporal region, and was accompanied by similar symptoms; and of a woman, aged 22, who presented atrophy of the thoracic parietes immediately below the right breast, without any trace of paralysis; and the analogous case by Stilling[a], —we shall have collected all the records which science possesses in reference to this form of partial atrophy. No other fact of the same kind, so far as I know, exists, even imperfectly observed.

I will not attempt to conceal the insufficiency of the few cases of which I have given an accurate abstract, nor the diversities it is easy to foresee united in a striking analogy; still more will I avoid drawing from them even the vaguest conclusion. It has appeared to me useful to bring together the only known examples of a lesion strange enough to attract attention, while I leave to the ingenious Berlin professor all the merit, but also all the scientific responsibility, connected with the subject.—*Archives Générales de Médecine*, May, 1852, p. 71.

Case of Progressive Muscular Atrophy. Reported by M. Gallard, Clinical Clerk at the Hôpital de la Pitié.

The most striking symptoms presented by the patient (a man of thirty-eight years of age, who had three years before taken up the

[a] Ueber Spinal Irritation, p. 325.

business of house-painting, at which he continued for twelve months), at the time of his admission into hospital under M. Valleix, were, continued irregular convulsive movements of the left arm. These movements had at first been painful, but had latterly ceased to be so. On more careful examination, they were found to be referrible to intermittent contractions of the triceps, and almost exclusively of the external portion of this muscle, which formed a considerable projection under the skin of the external and posterior part of the arm, while in the other two portions of the muscle, spasms of a few distinct fibres were alone visible. The biceps presented no evident contractions, but, when compared with the corresponding muscle of the opposite side, was found to be reduced to a fourth or a third of its normal bulk. The deltoid was completely absent, or at the utmost only its most posterior fibres were to be found, and these were likewise affected by spasmodic movements. The projection usually formed by this muscle on the shoulder did not exist; the head of the humerus could be felt immediately under the skin, and the external surface of this bone could be easily traced down to its middle portion; similar movements affected some of the digitations of the serratus magnus, as well as the external and posterior edges of the trapezius, particularly the fasciculi which are attached to the dorsal vertebræ.

The fore-arm was moved as a whole by the continual action of the muscles of the arm, but retained its normal bulk. Some very feeble movements were observed in the flexor carpi radialis and the supinator longus.

The motions produced by the triceps were very weak, those caused by the biceps were scarcely perceptible, and those of the deltoid completely lost. Thus, the patient could bring his arm backward and extend it; he could also bend it, although with difficulty; he could scarcely raise it forwards, and in order to accomplish this movement he was obliged previously to bend it at a right angle; lastly, he was wholly unable to move it outwards,—a motion for which the action of the deltoid is absolutely necessary.

Electricity, which had no action on the deltoid region except on its posterior part, produced slight contractions of the biceps, while it developed them very strongly and painfully in the triceps.

On the right side, some spasmodic movements were visible in the upper fibres of the pectoralis major; the right upper extremity presented nothing remarkable; in the lower, decided spasms existed, especially of some of the fibres of the sartorius, and also of some of the muscles of the inner and anterior part of the thigh. These spasms were well marked on the left side only. The measurement of the left thigh, compared with the right, showed an average diminution of about half-an-inch. All the ordinary functions of the system were regularly performed.

The author considers it evident that this case was one of the affection described by Dr. Aran under the name of progressive muscular atrophy. It was not, he observes, one of paralysis produced

by the influence of lead, for the corresponding parts of the limbs of both sides would then have been similarly affected at the same time; and besides it has been proved, by the ingenious experiments of Dr. Duchenne, of Boulogne, that muscles under the poisonous effects of this metal are insensible to the action of electricity. The history of this case showed that the paralysis was not the consequence of rheumatism. The patient had no symptom which could be referred to hysteria; and this affection is, moreover, very rare in men: his disease could therefore not be denominated hysterical paralysis,—an affection in which the muscles preserve their contractility under the application of electricity. Lastly, if it had been paralysis, the effect of lesion, or functional derangement of the brain or spinal marrow, all the muscles of the affected limbs would be simultaneously paralyzed to the same degree, and not in succession, as in the case described by the author.—*L'Union Médicale*, 16th March, 1852, p. 131.

Case of Unilateral Atrophy of the Face. By William D. Moore, A.B., M.B.

I have been induced, with the concurrence of the Editor, to translate Dr. Lasègue's paper, and to draw up the foregoing abstract of M. Gallard's observations, for the pages of this Journal, in consequence of having seen with Dr. Charles Johnson, in January, 1851, a gentleman, then aged 19, who presented symptoms which corresponded in many respects with the descriptions given by these writers. In this case, a small spot appeared, eight years previously to the above date, on the chin, to the left of the mesial line, and gradually extended, forming a hollow space. Some years subsequently, similar depressions took place on the forehead, cheeks, and upper part of the neck or rather throat, immediately below the jaw. At the time he consulted Dr. Johnson, the depression on the chin was deep, wrinkled, and of a dark brown colour; that on the forehead was about three inches long, by from one-half to three-fourths of an inch wide; all were on the left of the mesial line. The right side of the face was plump and well shaped, the left, even in the parts which were not absolutely hollowed out, was much thinner. The opening of the left nostril was, in consequence of the attenuation of its boundaries, considerably larger than that of the right. The tongue, uvula, and masseter muscle were unaffected. Sensation and motion were unimpaired, the sensibility of the most atrophied portions of the face even appeared to be increased, so much so that while the third power of an electromagnetic machine could be well borne in other parts, the first only could be endured in these spaces. It appeared to us, however, that this apparent increase of sensibility was not owing to any change in or excitement of the nerves, but to the fact of their extremities being less protected, in consequence of the loss of so much of their natural covering. A lock of hair near the affected part of the forehead, and this alone, was perfectly gray, the rest retaining its natural

brown colour. The patient's health was in all other respects perfectly good.

In this case the morbid changes appeared to be entirely confined to the parts supplied by the portio dura nerve of the left side; in fact, the emaciation seemed to demonstrate the distribution of this nerve, so much so, that on first seeing the case, Dr. Johnson remarked that it would appear to verify the observation of a writer, whose name he had forgotten, that the nerves of the human body had probably a third function in addition to the production of sensation and motion, viz., that of determining the nutrition of the parts they supplied; and that we might infer that in this case the portio dura had become paralyzed as to its office of exciting nutrition, while its motor power continued unaltered[a]. The treatment recommended was the use of cod-liver oil and the daily application of electro-magnetism, the ball connected with one wire being held as near as possible to the stylo-mastoid foramen, while the other was passed over the ramifications of the facial nerve. Our patient soon left Dublin, in consequence of which the plan laid down for him was but very irregularly adhered to; however, when we next had an opportunity of seeing him, in June, 1852, not only had the deformity not increased, but it appeared to us to be somewhat less than before. Over-exertion in lifting heavy weights had brought on an attack of hemoptysis, without cough or other alarming symptoms. In all other respects he was in the enjoyment of excellent health.

Dr. Aran, in the valuable papers alluded to by Dr. Lasègue[b], observes that the disease under consideration differs from atrophy properly so called, inasmuch as in the latter affection the muscular tissue, while it becomes reduced in volume, does not entirely disappear, and retains for a long time its contractile property, whereas in the former, the muscular fibre disappears and is probably replaced by cellulo, or fibro-fatty tissue,—probably, he says, for he is only aware of a single instance in which an autopsy was obtained, and in this case the affected muscles, which had, during life, ceased to obey the stimulus of electricity, were found to present no trace of fleshy fibre, but to be transformed into an adipose aponeurotic tissue[c].

Dr. Aran quotes cases which appear to be instances of this variety of muscular atrophy from the works of Charles Bell[d], Abercrombie[e], Graves[f], and Darwall[g], and shows that these writers all agree in attributing the disease to local affections of the nerves. He

[a] The writer alluded to by Dr. Johnson was Dr. Darwall, who, in treating of the same subject, observes that Dr. Wilson Philip's experiments "prove that secretion is under the superintendence of the nerves; and if secretion, then also nutrition must be concluded to be equally so."—*London Medical Gazette*, vol. vii. 1831, p. 203.

[b] Archives Générales de Médecine, September and October, 1850.

[c] Pages 28 and 193.

[d] On the Nervous System, p. 160.

[e] Pathological and Practical Researches on Diseases of the Brain and the Spinal Cord. Third Edition, p. 428. Edinburgh: 1836.

[f] Clinical Medicine, edited by Neligan, vol. i. p. 509.

[g] London Medical Gazette, vol. vii. p. 201. 1831.

afterwards gives the histories of ten cases witnessed by himself, and of one published three years previously by Dr. Dubois. As the case described by M. Gallard has many features in common with those of Dr. Aran, I have preferred giving an abstract of it to selecting one of the latter as a type of the rest, because by so doing I bring forward the testimony of an additional observer in reference to this singular disease. At the end of his paper Dr. Aran deduces the following conclusions from the cases he had observed:—

"1. The muscular system may be the seat of an atrophy with alteration of nutrition, and probably with cellulo-adipose transformation of the muscular fibre, independently of any [perceptible] lesion of the central or peripheral nervous system, and of any diminution in the circulation.

"2. This atrophy may be partial, being localized in a greater or less portion of the muscles of the upper extremities, or it may be general, extending to almost the entire of the muscular system of the life of relation; but under whatever form it occurs, it presents this curious circumstance,—that in the affected limb, certain muscles will remain perfectly intact, although surrounded by many others which are destroyed and transformed.

"3. This disease generally commences in the superior extremities; it is first characterized by weakness, then by emaciation of the limb, or of the affected portion of the limb, by spasms, startings of the tendons, and contractions of particular fibres or bundles of fibres. The termination of this morbid process is the complete destruction of the diseased muscles, and most probably their transformation into a cellulo-adipose tissue.

"4. This disease sometimes comes on spontaneously, without any appreciable cause, sometimes in consequence of occupations requiring the forced and continued action of certain muscles. It usually attacks young, robust, and healthy individuals, in whom it always constitutes, however limited its extent may be, a formidable infirmity.

"5. Its duration is generally long, its progress slow and gradual, almost invariably inducing the complete destruction of the affected muscular tissue. It seldom occurs that it remains confined to the muscles first attacked; most frequently it extends beyond them in the same limb, or shows itself in the corresponding portion of the opposite extremity.

"6. So long as the substance of the affected muscles has not been entirely destroyed and transformed into cellulo-adipose tissue, the muscular fibre preserves its irritability and its sensibility to the action of electricity; this character is valuable, for it may serve to distinguish the progressive muscular atrophy from some affections to which it bears affinity, particularly from the progressive paralysis without mental alienation, and from paralysis consequent on organic lesions or wounds of nerves.

"7. When the transformation of the muscular tissue is complete, no treatment can restore its integrity; before this period we may

hope to arrest the progress of the disease by acting on the muscular fibre, chiefly by means of localized galvanization."

In the wasting of limbs which occurs in cases of ordinary paralytic affections, the atrophy appears to depend upon the loss of function experienced by the muscles, or at most, the loss of power and of nutrition proceed simultaneously from an arrest or diminution of nervous influence. In the progressive muscular atrophy, on the other hand, the loss of power evidently proceeds from the absorption or destruction of the great bulk of the muscular fibre; in other words, it is the consequence and not the cause of the atrophied condition of the limb, and if the disease occurs in a class of muscles which are not habitually employed in overcoming considerable or varying resistance, and whose power consequently cannot be very easily tested, as is the case with the muscles supplied by the portio dura of the seventh nerve, it may make very great progress before any impairment becomes appreciable. Thus in the case I have detailed, there is no evidence of diminished muscular energy. In fine, while, in accordance with Dr. Lasègue's truly philosophical caution, we must avoid deducing general conclusions from a limited assemblage of facts, analogy would lead us, *à priori*, to infer that the ultimate step in the process of nutrition, namely, the reparation of tissue in every part of the body, must, like all other vital processes, be carried on under nervous influence, and that it may be possible for this particular function of the nervous system to become suspended without any simultaneous essential change in sensation and motion.

The most rational plan of combating this serious affection would appear to be a combination of local stimulation of the nerves, by means of some form of electricity as recommended by Dr. Aran, with the use of cod-liver oil, which has been found to exercise in other cases such powerful and beneficial influence upon nutrition, employing at the same time means calculated to maintain a good state of the general health, as residence in a pure atmosphere, sufficient exercise in the open air, avoiding over-fatigue; the use of a nutritious diet, and perhaps, under favourable circumstances, a course of sea-bathing.

As to the *prognosis* in this affection, Dr. Aran observes, that progressive muscular atrophy is, even in the most simple cases, a formidable disease; formidable as an infirmity, when it is confined to a very limited number of muscles, and shows but little tendency to spread; formidable even to life, when, by its rapid extension, it affects muscles which assist in carrying on a vital function, such as respiration. As an infirmity there is, perhaps, no disease which is so much above the resources of art; and, what is still more to be deplored, it generally attacks men in the vigour of life, who are industrious, addicted to toil, and dependent on their arms for their existence. As to a fatal termination, it is certainly very rare; and when it does occur, death is, generally speaking, not the direct effect of the disease itself, but the patients are carried off by some intercurrent affection.

Its causes are very obscure. On this subject Dr. Aran observes, that unremitting and excessive labour, and the prolonged contraction of certain muscles, are circumstances which may play an important part in the production of progressive atrophy; but it is impossible to see in these more than occasional causes of the disease. The number of individuals who pursue the same professions as those who have been the victims of the affection, and who labour with as much intensity as they have done, and who, however, exhibit nothing similar, is immense; but if there be predisposition (and in what this predisposition consists, or whether there be means of preventing it, we cannot tell), the limbs which are most exposed to fatigue will certainly be the first to suffer.

The disease which most closely resembles the progressive muscular atrophy, and with which, consequently, it may be most easily confounded, is, according to Dr. Aran, one described within the last few years, and as yet but little known, namely, progressive paralysis without mental alienation. In fact, this paralysis, like the atrophy, is progressive: it, too, affects a great portion of the muscular system, induces a rapid emaciation of the muscles, and in some cases even causes their disappearance. It is, however, distinguished from it by sufficiently well-marked characteristics: it commences in the lower extremities, is accompanied by numbness and tingling in the feet, seldom by cramps; is soon followed by true paralysis in the lower extremities; afterwards affects the upper extremities, where it likewise commences with tingling and numbness, followed by paralysis; at a later period produces difficulty of articulation (but this latter phenomenon is not constant), and induces death by a paralysis, which gradually ascends to the respiratory muscles, without having in the least involved the intellectual or sensorial faculties. It is also sometimes accompanied by anæsthesia or hyperæsthesia of the skin; but most generally there is diminution of muscular sensibility in the muscles, which have not undergone the slightest alteration of nutrition. There is no parallel to this in the progressive atrophy: there may be weakness connected with the muscular atrophy, but there never is paralysis. The disease generally commences in the upper extremities to which it long continues restricted, extending subsequently, but not always, to the lower, in which there never is paralysis properly so called unless the muscular fibre is completely destroyed, and replaced by adipose areolar tissue (tissu cellulo-graisseux). There is never anæsthesia or hyperæsthesia of the skin, nor is the muscular sensibility ever weakened. Lastly, a completely distinctive test has resulted from the recent researches of MM. Duchenne and Brierre de Boismont, namely, that the electric current brought to bear in the commencement on paralyzed muscles produces, in the general progressive paralysis, either none, or but feeble contractions of the muscular fibres, although the latter may have preserved their normal volume and elasticity, and even some voluntary movements; while in the progressive atrophy, so long as a single fibre remains

in the muscle to be acted on by the electrical excitant, it contracts. When the electricity ceases to excite contraction, it is because there are no longer any fibres to be acted on.

On the Combination of Lithotrity with Cystotomy. By Dr. Pietro Pagello.

In the "Gazzetta Medica Italiana," for December 1, 1851, is contained a paper by Professor J. E. Petrequin, of Lyons, on a mode of combining the operations of cystotomy and lithotrity in the extraction of large-sized calculi, so as to avoid the necessity of making a very extensive opening; and the observations are illustrated by the histories of two cases successfully treated in this novel manner.

Credit is due to the French surgeon for making lithotrity auxiliary to cystotomy; but, as I have already shown in the Number of this Journal for last June, that priority[a] in the employment of lithotrity instruments in the extraction of bodies which have accidentally fallen into the bladder belongs to the hospital of Belluno, I shall now be able to prove that the same institution set the example of uniting the two operations alluded to; and that from 1798, an operation for the stone was employed by Dr. Marchi with the lateral incision, followed by drilling the stone,—anticipating Civiale's method,—and by percussion with a hammer, as taught by Heurteloup so many years after.

I shall now describe Marchi's operation in his own words, adding a short practical observation of my own on large vesical calculi, deduced from my practice in the Civic Hospital at Belluno.

Case.—"The lateral operation for lithotomy was performed on Valentine Urban, aged twenty-three years, on the 14th September, 1798. The prehension of the stone was at first found to be difficult; but by the introduction of the finger into the rectum, and the thereby elevation of the *bas fond* of the bladder, the calculus was rendered accessible to, and firmly seized with, the forceps; every attempt to extract it proved, however, unavailing. It was very hard, and studded in every part presented to the touch with thorn-like elevations; by no degree of force could it be moved, even to the extent of a line. The contractions of the bladder, and of the muscles around the incision, were the great obstacle. I determined to dilate the wound as much as the parts would admit of, and to form an opening sufficient to allow the removal of a larger but smooth calculus: all was, however, to no purpose. The contractions were so great, and acted so simultaneously in narrowing the course of the wound, and in drawing back the stone when the extracting force was diminished, and their remissions were so short, that not an instant could be made available to overcome the difficulty;

[a] Lithotrity would appear to have been practised in England so early as the year 1559. See Sir Philip Crampton's paper in the first volume of the present series of this Journal, pp. 13, 14.

on the contrary, the state of spasm was aggravated every time the stone was pressed with any force against the inner part of the wound. The impossibility of removing the calculus by the ordinary method made me determine to drill it, instead of using the breaking forceps of Lecat, or of having recourse to the operation above the pubis, or of waiting for the relaxation which attends suppuration in order to accomplish its extraction.

"The operation was performed in the following manner:—I took a steel drill six inches long, and large enough to make an opening of three and a half lines; on the moment, I made use of what is technically termed 'a bit and brace'; with my right hand I retained the stone in the forceps, and with three fingers of the left I kept the drill so directed as not to press upon the sides of the incision. An assistant with one hand held the brace, and with the other turned the bow. There was some little difficulty in making an impression on the stone, on account of the hardness and inequality of its surface, and because the instrument I used, being composed of several pieces, was not so manageable at that which has a single shaft. Having made a hole about an inch deep, I caused the brace to be removed, transferred the forceps to the care of an assistant, and struck a slight blow, followed by two others more strongly given, with the hammer on the drill, when the stone separated into two nearly equal pieces, besides four smaller fragments. I became aware of the division of the stone from feeling the loss of resistance, and from finding that the drill easily passed into the line of division of the two pieces. The drill having been removed, I succeeded in separating the two portions with the index finger of the left hand, so as to admit of the withdrawal of one. The first came out easily, and without any inconvenience to the patient. Having then again made a passage with my index finger for the forceps, I removed the second. I washed out the bladder with upwards of four pints of tepid water, which brought away the four little fragments. The hardness of the stone was so great that the course of the drill was smooth and polished. The patient subsequently suffered from pains and swelling of the abdomen,—symptoms not usually experienced by those I operate on. These symptoms were allayed, after thirty hours, by means of venesection, opiates, a gentle purgative, fomentations, and lavements, and in twenty-three days the patient left his bed completely cured."

This interesting case made me for many years wish to combine lithotrity with cystotomy, if a similar case should occur to me; but at the present day operators for the stone are numerous compared to what they were in the last century, and, consequently, few calculi are allowed to attain a large size. Among thirty-four calculi which I operated on according to Frère Côme's method, I only met two very large in adults, and these were so enormous as to prevent their being embraced by the largest lithotrites of Civiale and of Heurteloup which I possess: one weighed four ounces, the other five and a half ounces (Vienna medical standard); they were, however,

smooth on the surface, and, being strongly seized with the forceps, by patience and by slowly dilating the incision by traction of the calculus, I succeeded in disengaging them after the assistants had despaired of success.

Such traction, practised with gentle steadiness, and with a vibrating movement while the perineum is supported, leaves in the wound a certain element of reparative contractility, in virtue of which, with the assistance of the application of cold, the aperture acquires, after a few hours, nearly ordinary dimensions. In the case, however, in which the stone weighing five and a half ounces existed, I had to deplore the formation of a short recto-vesical fistula, which I cured after some months by the application of potential caustic on the anal side, a large staff having been previously placed in the urethra; and I chose this mode because I have no reliance on suture. I found Lisfranc's speculum to answer well in the application of the caustic.

I have one short observation to add on the formation of vesical calculi, viz., that forty which I removed at Belluno in the space of fifteen years by lithotrity or cystotomy, without losing a single patient, were all from peasants, shepherds, and woodcutters, and thirty-two of these individuals inhabited districts where the water ran over a red, calcareous earth,—a fact which my want of chemical knowledge renders me unable to explain, and which I would therefore simply submit for the opinion of those practically acquainted with the subject.

In conclusion, I wish to point out how much more simple is the plan of removing a calculus in a single piece whenever it is at all practicable, and what large stones may be extracted through incisions made with the bistouries of Frère Côme.—*Giornale Veneto di Scienze Mediche*, January, 1852, p. 51.

[It seems now to be universally admitted by the modern surgeon, that one of the sources of death from lithotomy is diffuse inflammation of the areolar membrane of the pelvis, frequently the result of too bold a division of the neck of the bladder,—and, indeed, of these dangers nobody seemed more aware than the author's illustrious countryman, Scarpa. The author proposes to obviate the danger by combining the operations of cystotomy and lithotrity. As many emergencies may arise which might render this combination useful, we think the subject, on several accounts, deserving of the consideration of the practical surgeon in cases where large stones are encountered. We cannot but commend the spirit of the author in claiming for his countryman the merit of priority in the employment of lithotrity instruments; but neither his observations upon this point, nor the solitary fact that an operation of breaking the stone was performed in England so early as the year 1559, should detract from the merit of bringing lithotrity into general use, which is evidently due to Civiale, nor lessen that of Heurteloup, who brought percussion of the calculus with the hammer into favour.

Although these severe operations may be not only endured with impunity, but attended with happy results, as in the author's forty cases of "peasants, shepherds, and woodcutters," we surgeons of a metropolis nevertheless think it right to warn our younger brethren not to anticipate happy results from similar operations performed on the careworn denizens of a crowded city, in whom too frequently the vesical calculus is symptomatic of renal disease.—Ed.]

On the Use of Collodion to prevent Pitting of the Face in Small-Pox. By M. Quarrin Willemier, Officer of Health of the First Class in the Dutch Army.

This practitioner has successfully employed collodion in a case of modified small-pox, in which the pustules were confluent on the face. On the first day of the eruption he applied on the cheek a slight layer of collodion, which continued adherent until the period of desiccation. As soon as the collodion began to be detached it was removed, when the subjacent skin was found to be perfectly smooth and in its normal condition, while the surrounding portions of the integuments presented crusts and slight depressions. The patient stated that he had felt no itching where the layer of collodion had rested, while this sensation was constantly present in the neighbouring parts.

The means hitherto employed to prevent pitting from small-pox are the following:—Cauterization of the pustules with nitrate of silver, the application of Vigo's plaster (emplastrum hydrargyri cum ammoniaco), or of a constantly renewed layer of mercurial ointment. The first of these is an ectrotic plan, for the vesiculo-pustular secretion is at once discharged by it; the other two should rather be called resolutive means. If the layer of mercurial ointment be applied but for a day or two, the purulent secretion is only retarded, and will re-appear. In order with certainty to prevent its formation, the layer of ointment must be kept up until the desiccation of the last pustule on the body.

Now, if it were proved that Vigo's plaster and mercurial ointment merely acted by protecting the eruption from the contact of the air, and perhaps of the light, it would be much simpler to cover the face with diachylon, with lard, or, as the author has done, with collodion, and thus avoid the danger of salivation. But a decisive experiment has already shown that diachylon and lard are inefficacious. In 108 soldiers affected with small-pox, M. Champouillon covered the face with Vigo's plaster, while diachylon, lard, or a layer of oil was applied to the neck. Except in 11 cases, the Vigo's plaster prevented the pustular eruption, while only four patients escaped the formation of pustules on the neck.

Might a more fortunate result be expected from the use of collodion than M. Champouillon has obtained with diachylon, lard, or oil? A single fact is not sufficient to decide this question; for the means just quoted have succeeded in nearly five per cent. of the

cases in which they were tried, while the almost constant efficacy of topical mercurial applications leads to the inference that the latter possess a specific action; at the same time, nothing can be easier or safer than to institute experiments with collodion similar to those of M. Champouillon.

The selection of a preventive for variolous pits may be based on the following considerations:—The cauterization of the pustules, otherwise called the ectrotic method, is little employed; the application of the caustic is a minute and difficult operation, for each vesicle must be separately opened and cauterized with a fine pencil of nitrate of silver; it is, moreover, very painful; it ought at the utmost to be confined to the pustules on the eyelids, which it is very difficult to cover accurately with Vigo's plaster. The cauterization *en masse* of the face with a concentrated solution of nitrate of silver has likewise been proposed, but is also very painful, and is little to be recommended. The mercurial topical applications are greatly preferable to the foregoing; it is asserted that, far from producing pain, and, consequently, a swollen and dangerous state of the face, they prevent congestion of this part, and are even observed to allay existing cerebral symptoms. Lastly, the mercurial ointment is more eligible than Vigo's plaster, because the latter cannot in all instances be evenly applied to the surface, and, in the case of children, it cannot without great difficulty be kept in its place, while, with a little attention, the face can always be kept smeared with a layer of ointment. The only inconvenience of these mercurial applications is the occasional production of salivation; but this sometimes occurs spontaneously in small-pox, and topical mercurial applications have not been found to aggravate it. A more decided inconvenience produced by the use of these local remedies is, the eruption of mercurial eczema; but even this is a mild affection, and, like salivation, not to be compared to the formidable results of confluent pustules on the face. It must in addition be observed, that it is, at least in theory, prudent to assist the coming out of the eruption on the limbs whenever that on the face is treated ectrotically; for this purpose, warm poultices to the feet will be found useful.—*Repertorium, Tijdschrift voor de Geneeskunde—Nouvelle Encyclographie des Sciences Médicales*, April, 1852, p. 253.

Special Pharmacopœia for Diseases of the Skin. By Alphée Cazenave, M. D., Physician to the Hôpital St. Louis, Paris.

(*Continued from* Vol. XIII. p. 504.)

Arsenic.—Arsenic, the therapeutic use of which appears to have been from the earliest ages diffused generally throughout India and China, has been, since the sixteenth century, much employed by the physicians of Germany, England, France, and Italy.

The therapeutic history of this heroic remedy is one of the most interesting on record[a]. Thousands of facts had long attested the

[a] Dictionnaire de Médecine, tome iv., Art. Arsenic. Paris: 1833.

efficacy of arsenical preparations in the treatment of intermittent fevers, when experience proved that they might be also advantageously employed in diseases of the skin; it was, moreover, known that arsenic had been extolled in India as a remedy in the most serious affections, and particularly in elephantiasis.

In 1817, Biett, repeating at the Hôpital St. Louis the experiments he had just witnessed in the London hospitals, introduced into France the employment of arsenic in the treatment of cutaneous affections, and from the first laid down rules for its use which it has not since been found necessary to modify.

The preparations of arsenic, notwithstanding their incontestible value in the treatment of many of the diseases of the skin, are not applicable to all. They are principally useful in the essentially chronic affections; in the dry forms, such as psoriasis and lepra; and in eruptions of another class, which, after having resisted rational treatment, and having become, as it were, established, show an obstinacy which seems dependent on local, idiopathic conditions of the skin, as in chronic lichen, and especially in certain forms of eczema.

They afford us the most effectual remedies in the treatment of the elephantiasis of the Greeks.

Good effects have also been obtained from the employment of arsenical preparations in the treatment of venereal diseases, and especially in the tuberculous and scaly forms of syphilitic eruptions.

I have already mentioned, that the efficacy of certain decoctions (de Felz, d'Arnault, &c.), in the composition of which more or less antimony is employed, is generally attributed to the presence of arsenic.

Iodide of Arsenic.—*Thomson's Pills.*—Iodide of arsenic, three-fourths of a grain; extract of hemlock, one scruple; make into ten pills,—*one* to be taken every eighth hour. This very active formula has been principally employed in lepra.

Mr. Donovan has recommended the double iodide of arsenic and mercury, a compound of equal parts of iodide of arsenic and biniodide of mercury, in the treatment of lepra, psoriasis, lupus, and of syphilitic affections. *Solution of double iodide of arsenic and mercury modified by Soubeiran.*—Iodide of arsenic, iodide of mercury, of each, one part; distilled water, ninety-eight parts. This solution contains an hundredth part of each iodide.

Arsenious Acid.—*Liquid Arsenious Acid.*—A solution of three-quarters of a grain of arsenious acid in eight ounces of distilled water is employed under this name in the German hospitals. The dose is one table-spoonful, gradually increased to six, and is to be taken in the morning on an empty stomach (Foy). *Dr. Gibert's Formula.*—Arsenious acid, three-quarters of a grain; distilled water, sixteen ounces; dissolve by the aid of heat; divide into five phials, the contents of each of which are to be taken in the morning of either one or two days, according to circumstances. This formula is generally preferred by M. Gibert in the treatment of dis-

eases of the skin, and especially of psoriasis. *Asiatic Pills.*—Arsenious acid, three-quarters of a grain; black pepper, ten grains; gum Arabic, one-sixth of a grain; water, as much as may be sufficient to make twelve pills,—one to be taken daily. This preparation is very active; it is the one I usually prefer, varying the dose from one to two, but more frequently diminishing it in the following manner:—Take of the mass for Asiatic pills, eight grains; extract of taraxacum, twenty-three grains; mix, and divide into twenty pills, of which one or two are to be given daily.

Arsenite.—But one arsenite is employed in therapeutics, namely, the arsenite of potash, forming the basis of Fowler's solution, which is a very energetic medicine, and requires to be used with caution. Biett, who frequently prescribed it, recommends that the daily dose should at first be only two or three drops, and never exceed twelve, administered in two equal portions.

Arseniates.—*Pearson's Arsenical Solution.*—Arseniate of soda, three-quarters of a grain; distilled water, one fluid ounce; dissolve and filter (Codex). Dose: a scruple, gradually increased to half-a-drachm, and even a drachm. This preparation is milder and more manageable than Fowler's. I prefer it with women and children, in recent cutaneous diseases. *Biett's Arsenical Liquor.*—Arseniate of ammonia, three grains; distilled water, four fluid ounces. The uses and dose of this are the same as those of the preceding preparation. *Pills of Arseniate of Soda.*—Arseniate of soda, three-quarters of a grain; extract of taraxacum, half-a-drachm; divide into thirty pills,—one or two to be taken for a dose. *Use*, the same as of the preceding preparation. I often employ this formula with advantage. *Pills of Arseniate of Iron* (Biett).—Arseniate of iron, two and a half grains; extract of hops, one drachm; marsh-mallow, powdered, half-a-drachm; syrup of orange-flower, a sufficient quantity; make into forty-eight pills. Biett employed these pills particularly in scaly affections and in lupus,—the dose is one daily.

Elecampane.—One of the Compositæ, the root of which has been advantageously employed in the treatment of some cutaneous diseases as a bitter, tonic, and diaphoretic. Besides an acrid resin, it contains about 37 per cent. of bitter extractive. *Elecampane Ptisan.*—Elecampane-root, one ounce; boiling water, one quart; infuse, strain, and add one ounce of syrup of capillaire. The infusion is very aromatic, and is administered internally in scrofulous cases, in impetigo, and in chronic eczema; it is also well adapted for children of lymphatic temperament, and for young females in whom the catamenia are irregular. The decoction being more acrid is more particularly suitable for external use; it is employed in lotions, or applied on compresses in the same affections, and especially to allay itching.—*Annales des Maladies de la Peau et de la Syphilis*, tom. iv. 1852.